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PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

VOL. LXIX. For the Year 1779~

PART I

LONDON,

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MDCCLXXIX.



A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL
TRANSACTIONS.

- I. *An Account of a Cure of the St. Vitus's Dance by Electricity. In a Letter from Anthony Fothergill, M. D. F. R. S. at Northampton, to William Henly, F. R. S. Communicated by Mr. Henly.*

S I R,

Northampton,
Oct. 28, 1778.

Read Nov. 5,
1778.

AGREEABLE to my promise, I now proceed to give you some account of a recent cure performed by electricity, which will, I think, afford you much pleasure.

VOL. LXIX.

B

ANN

ANN AGUTTER, a girl of ten years of age, of a pale, emaciated habit, was admitted an out-patient at the Northampton Hospital on the 6th of June last. From her father's account it appeared (for she was speechless and with difficulty supported from falling by two assistants) that she had for six weeks laboured under violent convulsive motions, which affected the whole frame, from which she had very short intermissions, except during sleep; that the disease had not only impaired her memory and intellectual faculties, but of late had deprived her of the use of speech.

Volatile and fetid medicines were now recommended, and the warm bath every other night; but with no better success, except that the nights which had been restless became somewhat more composed. Blisters and anti-spasmodics were directed, and particularly the flowers of zinc, which were continued till the beginning of July, but without the least abatement of the symptoms; when her father growing impatient of fruitless attendance at the hospital, I recommended, as a dernier resort, a trial of electricity, under the management of the Rev. Mr. UNDERWOOD, an ingenious electrician. After this I heard no more of her till the first of August, when her father came to inform me that his daughter was well, and desired she might have her discharge. To
I which,

which, after expressing my doubts of the cure, I consented; but should not have been perfectly convinced of it, had I not received afterwards a full confirmation of it from Mr. UNDERWOOD, dated Sept. 16, an extract from whose letter I will now give you in his own words.

“ I have long expected the pleasure of seeing you,
“ that I might inform you how I proceeded in the cure
“ of the poor girl. As the case was particular, I have
“ been very minute, and wish you may find something
“ in it that may be useful to others. If you think it proper,
“ I beg you will state the case medically, and make
“ it as public as you please.

“ July 5. On the glass-footed stool for thirty minutes:
“ sparks were drawn from the arms, neck, and head;
“ which caused a considerable perspiration, and a rash
“ appeared in her forehead. She then received shocks
“ through her hands, arms, breasts, and back; and from
“ this time the symptoms abated, her arms beginning to
“ recover their uses^(a).

“ July 13. On the glass-footed stool forty-five minutes:
“ received strong shocks through her legs and
“ feet, which from that time began to recover their
“ wonted uses; also four strong shocks through the jaws,
“ soon after which her speech returned.

(a) The coated bottle held near a quart.

“ July 23. On the glass-footed stool for the space of
“ one hour: sparks were drawn from her arms, legs,
“ head, and breast, which for the first time she very sen-
“ sibly felt; also two shocks through the spine. She
“ could now walk alone; her countenance became more
“ florid, and all her faculties seemed wonderfully
“ strengthened, and from this time she continued mend-
“ ing to a state of perfect health.

“ Every time she was electrified positively, her pulse
“ quickened to a great degree, and an eruption, much
“ like the itch, appeared in all her joints.”

Thus far Mr. UNDERWOOD. To complete the history of this singular case, I this day (Oct. 28) rode several miles, on my return from the country, to visit her; and had the satisfaction to find her in good health, and the above account verified in every particular, with this addition, that at the beginning of the disease she had but slight twitchings, attended with running, staggering, and a variety of involuntary gestimations which distinguish the St. Vitus's dance, and that these symptoms were afterwards succeeded by convulsions, which rendered it difficult for two assistants to keep her in bed, and which soon deprived her of speech and the use of her limbs. The eruptions which appeared on the parts electrified soon receded, without producing any return of the symptoms,

symptoms, and therefore could not be called critical, but merely the effect of the electrical stimulus. Having given her parents some general directions as to her regimen, &c. I took my leave, with a strong injunction to make me acquainted in case she should happen to relapse. Before I conclude, it may not be improper to observe, that some time ago I was fortunate enough to cure a boy who had long had the St. Vitus's dance (though in a much less degree) by electricity. A violent convulsive disease, somewhat similar to the above, though, if I recollect right, not attended with the *apbonia*, was successfully treated in the same way by Dr. WATSON, and is recorded in the Philosophical Transactions. May we not then conclude, that these facts alone, and more might perhaps be produced, are sufficient to intitle electricity to a distinguished place in the class of antispasmodics?

I am, &c.

II. *A Case in which the Head of the Os Humeri was Jawed off, and yet the Motion of the Limb preserved.* By Mr. Daniel Orred, of Chester, Surgeon. Communicated by Thomas Percival, M. D. F. R. S. and A. S. and Member of the Royal Society of Physicians at Paris.

S I R,

Manchester,
Sept. 20, 1778.

Read Oct. 12,
1778.

A VERY eminent surgeon at Chester has desired me to transmit the inclosed case to the Royal Society, and I hope it will be deemed worthy of publication. It not only affords a confirmation of an important fact inserted in the Philosophical Transactions, vol. LIX. art. VI.; but shews also that the surgical improvement, proposed in that article by my ingenious friend Mr. CHARLES WHITE, may be extended to operations on other parts of the human body.

I am, &c.

THO. PERCIVAL.

A FRIEND

A FRIEND of mine, an ingenious surgeon, settled at Tarporley, in this county, sent for me about the middle of last month to see a patient of his, a gentleman's servant in that neighbourhood. This was a man of about forty years of age, who had much injured a good constitution by hard drinking before the following accident happened to him.

From an injury received more than three years before the time I saw him, by a fall from the top of a ladder, I found a suppuration had taken place in the shoulder joint. The matter had made its way through three small openings; one in the *axilla*, directly opposite to the cavity of the joint; the other two lower than the strong tendons of the pectoral muscle, and betwixt the *deltoides* and *biceps* muscles. Upon introducing a probe into the joint by the upper orifice, I found the head of the *os humeri* exceedingly carious. A few weeks before I saw him, a collection of matter had formed upon his foot, I suppose from an absorption and translocation from the shoulder. Upon letting out the matter with a lancet, I found the metatarsal bones also very carious: with these shocking complaints no wonder he was much enfeebled and reduced. As the disease in his shoulder would evidently soon have put a period to his life without immediate relief, I proposed to him,

him, either to amputate the arm at the diseased joint, or, with a view of making it of some use to him, endeavour to saw off the head of the affected bone only. As the least of two evils he chose the last; though this indeed is a most painful, hazardous operation. We are indebted to Mr. WHITE, of Manchester, for the mode of this operation, as well as for many other valuable hints and discoveries in surgery. In order to allow the arm as much action as possible after the operation, I began my incision a little above the joint, and continued it in a right line directly through the middle of the fleshy portions of the *deltoides*, and a little lower than its insertion: then elevating the arm to relax the muscle, an assistant with both hands distended the upper part of the opening made by the incision, whilst, with a narrow knife, I endeavoured, by the direction of the fore-finger of the left hand, carefully to divide the capsular ligament. This was effected with very great difficulty, as from preceding inflammation it was much thickened, and adhered closely to the joint: and till it was separated nearly round (for the under, and most dangerous part to cut through, was corroded with matter) I found my utmost efforts to throw the head of the bone out of its socket quite ineffectual. After a sufficient separation I made the dislocation, by pressing the elbow to the body with one hand, and with the

the other pulling the head of the *bumerus* directly towards me. After guarding the great artery against the action of the saw, by introducing a piece of paste-board under the bone, I separated it across, as Mr. WHITE directs, as low down as I possibly could to prevent an exfoliation. The loss of blood was very trifling. After dressing the wound very superficially, I took particular care that the artery was not pressed upon by the bandages; and advised, when the inflammation subsided, and a good digestion came on, that his arm should always be dressed when the body was erect, and suspended a little from it, with the fore arm a little bent: this was accordingly done. In a few days after the operation, he got up, and continued to sit up the day through ever after. He had a cold infusion of the Peruvian bark with the weak spirit of vitriol ordered him. In consequence of his very reduced habit of body, his shoulder was long in curing. A small exfoliation took place. The cure, I fancy, was also much retarded by the diseased foot, which still continued very bad; the man being so exceedingly terrified by the former operation, that he would not suffer us to do any thing to any purpose to it.

I saw him about three months after the accident. The wound was nearly cicatrized; but the ossification was not so far advanced as I expected it would have been, the

callus being much smaller than the lower part of the *bumerus*, and bending with the weight of the arm. However, he could raise it from his body more than could be well supposed, and had the perfect flexure and use of his fore arm. This case, with all the disagreeable circumstances attending it, strongly proves the utility of the above operation. By a similar operation diseases of other joints may be as easily cured.

About five years ago, a young man of the name of MOORES, about sixteen years of age, and son to a farmer at Aldersey, a village in this neighbourhood, applied to me with a disease in the lower part of the *ulna*, where it joins the bones of the *carpus*. A variety of means for several years had been tried to no purpose, to relieve him before he came to me. The patient was of a good habit of body, and seemed to enjoy tolerable good health. With his consent I carefully separated from the adjoining parts, and sawed off, more than three inches of the enlarged bone. A *callus* at a proper time formed in the intermediate space. He is now able to undergo the most laborious parts of husbandry business; though that part of the fore arm where the operation was performed is still something smaller than the same part in the other arm.



III. *Experiments on some Mineral Substances.* By Peter Woulfe, F. R. S. Communicated at the Desire of William Hunter, F. R. S. and Physician extraordinary to the Queen.

Read Nov. 19, 1778.

On crystal, quartz, and flint.

MONS. BEAUMÉ, a member of the Royal Academy of Sciences of Paris, has asserted, that he obtained allum from crystal, quartz, and flint. His method to obtain allum from these substances was to make a *liquor filicum*, by melting them in a crucible with fixed alkaly, and letting the mixture run *per deliquium*; any acid added to this *deliquium* precipitates the crystal, quartz, or flint, which, by means of acid of vitriol, he dissolved, and obtained allum. I have often repeated this operation on all these substances, but could never obtain a single grain of allum: in lieu thereof I got a selenite, and was on that account convinced, that the basis of these substances was a calcareous earth. Mr. BEAUMÉ's mistake was, I am certain, owing to the Paris crucibles which he

made use of, and which, for want of being well burnt, retained yet their clayey nature; consequently a portion, and that not a small one, of the clay, is dissolved by the alkaly; allum must therefore in his manner be obtained, as clay is the matrix, or at least contains the earth of allum. It is rather a tedious operation to make the *liquor silicum*, and especially in any quantity; for the mixture is very subject to froth and boil up in the crucible, and the operation is not compleat until this boiling up ceases, and the mixture melts thin. Hence it is natural to conclude, that a large portion of the clay of a Paris crucible is dissolved by the alkaly during this long operation.

In order to discover whether the crystal, quartz, or flint, contained any acid, I put in practice the method which I published in my last paper on mineral substances, to discover the acids of horn silver and horn mercury; but I could not find they contained any.

On the change of crystal into selenitical spar.

The crystal on which I tried the following experiments, and which is partly changed into selenitical spar, is to be found in that most excellent museum collected by Dr. HUNTER. This crystal comes from near Freyberg in Saxony, and is called in German *nieren-formiger cry-*

Real (kidney-formed crystal). It is in the form of an uneven crust, with small protuberances; a part of it retains all the appearances of crystal, but the rest is opaque, somewhat of an ash colour, and is easily powdered: the underpart retains marks of *galena* or potter's lead ore, and in some specimens the greatest part is coated with it.

I took three drams of the opaque part of this crystal finely powdered, and mixed it with an equal quantity of fixed alkaly of cream of tartar, and in order to facilitate the union, the mixture was made into a paste with distilled water, then dried and calcined for an hour as in my experiments on horn silver (see Phil. Transf. vol. LXVI. p. 604.). The mixture was then taken out of the phial powdered, and, by digestion with distilled water at three different times, deprived of its saline part: these three solutions were mixed together, saturated with distilled vinegar, evaporated to dryness and washed well with rectified spirit of wine. By this means 3j. and gr. vi. of tartar of vitriol was obtained. The undissolved part, remaining after digestion with water, when dried weighed 3 ij. and gr. xxiv.; this was mixed and digested with pure acid of nitre, and effervesced strongly. After this digestion the undissolved part here remaining, being well washed and dried, weighed 3 j. and gr. xv. which I take to be crystal, for no saline matter was obtained by treating
it

it with acid of vitriol; and hence it appears, that nearly two-thirds of the crystal is changed into selenitical spar. The solution which was obtained by the acid of nitre mixed with acid of vitriol, but especially with a solution of tartar of vitriol, precipitates copiously, and forms a selenite. This evidently proves the existence of the calcareous earth, and the tartar of vitriol mentioned above, that of the acid of vitriol in this altered crystal.

The unaltered part of this crystal, treated in the same manner as the altered part, afforded no marks of selenitical spar.

The change of this crystal is, I suspect, owing to the lead ore which adheres to it; the sulphur of the lead ore furnishing the acid of vitriol, and the crystal the calcareous earth.

I have in the above manner treated a variety of the heavy spars, commonly called selenitical or gypseous spars, and found them all, as I first judged by their appearance and great weight (without being once mistaken) to consist of acid of vitriol, with calcareous earth and some clayey matter. In some, the calcareous earth was combined with a larger portion of this acid than in others; and in some, the clayey matter was predominant. Among these were the following, some of which were not before taken for selenitical spars.

The Derbyshire and Eckton cauk, which is commonly covered with copper marcaffite.

Ditto from Saxony, which is found growing like knobs or protuberances on the yellow phosphoric spars.

The *spatbum ericæ forme* of Woodward from Derbyshire.

The stellated spar, which is found on the *ludus belmontii* from the island of Sheppy.

The spar from Saxony, resembling lead ore, and which by many is supposed to be one, called in German *flangen spatb.* This spar is by Baron BORN classed with the basaltés; he has no doubt taken it for a shirl, which is classed with the basaltés, but I never found them to have any affinity.

Another spar from Saxony, called also *flangen spatb.*, composed of large interwoven prisms, partly red and partly white, and semi-transparent.

Compact semi-transparent Auvergne spar, said by M. MONET to contain sulphur; but I could never find an atom of it in this or in any of the other spars I tried, and suppose his mistake to be owing to a portion of charcoal, flame of charcoal, or dust, that got into the crucible. Native sulphur is frequently found growing with calcareous spar and gypsum, but it should be considered as heterogeneous.

Flaky milk-white spar, found with Cobalt and copper ores from Saalfeld.

Two

Two spars from the Hartz, composed of flakes and crystallized.

Ditto from Saxony.

Striated spar from Scotland, brought here by Mr. GOAN.

Spar intermixed with cinnabar and ochre of iron from Obermoschel in the dutchy of Deux Ponts.

Red ochry compact spar from Derbyshire.

I shall give a few of my experiments on these substances compared with the common Paris montmartre plaster stone; all these were deprived of their humidity before they were submitted to trial.

Two drams of the Paris plaster stone, treated as in the former experiment, with an equal quantity of alkaly of tartar, produced $\frac{3}{4}$ jfl. and gr. viii. of tartar of vitriol^(a).

The insoluble part remaining after being deprived of its saline part by water, was digested with distilled vinegar, which effervesced and dissolved the whole of it, except a small portion, which, when washed and dried, weighed gr. vi. and had the appearance of tobacco-pipe clay. Had I taken a pure white transparent gypsum, I believe that no sediment would have been left. From

(a) The tartar of vitriol, of all the trials I make mention of in this paper, was dissolved in distilled water, and mixed with a solution of calcareous earth in acid of salt, which caused a copious selenitical precipitation, a clear demonstration of the existence of the acid of vitriol.

this

this experiment it appears, that gr. xcvi. of vitriol was produced by gr. cxiv. of pure gypsum, for the gr. vi. of clayey matter must be deducted.

The like quantity of Derbyshire cauk, treated in the same manner, produced gr. lix. of tartar of vitriol, and the clayey matter, which exactly resembled the former, weighed gr. xxiii.

The stellated spar from the island of Sheppy, submitted in the same proportion to the same trials as the foregoing, produced gr. lviii. of tartar of vitriol, and gr. xxii. of clayey matter, which was whiter than in the former.

WOODWARD'S *erica* formed spar in the like proportion and manner as the former, afforded gr. xxxviii. of tartar of vitriol, and gr. xlii. of clayey matter.

To shew that the calcareous earth in the gypsum contained a greater portion of acid of vitriol than that in the selenitical spars, I reasoned thus, having previously subtracted the clayey matter of each.

If 114 grains of plaster (the 6 grains of clay being deducted) afford 98 grains of tartar of vitriol, how much should 97 grains of the gypseous matter contained in cauk afford (for the clayey substance must also here be subtracted). From this rule it should produce $83\frac{1}{3}$ grains of tartar of vitriol, and nevertheless the quantity

was only 59 grains. Hence it is clear, that the calcareous earth of the gypsum contains a greater proportion of acid of vitriol than that of the cauk.

According to this rule, the stellated spar from Sheppy should have afforded $84\frac{14}{57}$ grains of tartar of vitriol, but it produced only 58 grains.

The *erica*, formed spar by the same rule, should have afforded $65\frac{17}{57}$ grains of tartar of vitriol, but the quantity was only 38 grains.

As I suspected that the selenitical spars contained some calcareous earth, not united to the acid of vitriol, I digested some of them with rectified pure acid of nitre, and afterwards well washed and dried them; but the loss of weight was very trifling, except in the cauk and *erica* formed spar; the first probably containing some marcasfital particles of copper, and the last an ochre of iron.

Dr. LEWIS, in his translation of NEWMAN's Chemistry, quotes from the Philosophical Transactions somewhat remarkable about cauk, that when wetted with antimony it gives it a shining surface like steel.

I repeated the experiment as follows: I powdered and mixed ℥ ff. of cauk with ℥ iij. of antimony, and put the mixture into a red hot crucible; and when melted, which readily happens, I stirred it with an iron rod, and poured

it

it into an iron mortar. This matter in its fracture has appearances of RULANDUS's false liver of antimony.

I have also tried, in the same proportion and manner, the following substances, and found the effects so much alike, that they could be scarcely distinguished one from the other.

Stellated spar, from the island of Sheppy.

Auvergne compact plated spar.

Erica formed spar.

Whited plated gypsum dried.

Dried whiting.

Fixed alkaly of tartar.

Hence we may conclude, that the calcareous earth of these spars and gypsum act on the antimony like fixed alkaly, forming a sort of liver of antimony.

Of some mineral substances which contain the earth of allum.

Tobacco-pipe clay, of all substances I know, would be the fittest to make allum with, was it necessary; but nature has supplied it abundantly in other bodies, from which it is obtained with little art and expence.

Two drams of dried tobacco-pipe clay, treated with an equal quantity of fixed alkaly of tartar, as in the last ex-

D 2

periments,

periments, produced no tartar of vitriol; nor have I been able to obtain any from the other clays, which I submitted to the same trials: such were the porcellane clay from Cornwall, the porcellane clay from Saxony, Sturbridge clay, fuller's earth, as also the blue argilla from Paris, which M. BEAUME says is replete with the acid of vitriol^(b), and to that he attributes its property of setting free the acids of salt petre and of sea salt; but of this more fully hereafter.

The tobacco-pipe clay, having been deprived of its saline part after the calcination, was as white as chalk, and had lost its tenacity^(c). It increased in weight gr. viii. and the alkaly was not only diminished in weight, but was also combined with a portion of the clay; for, on saturating it with distilled vinegar, a gelatinous substance was separated. I have often observed the formation of this gelatinous matter on the surface of the Vauxhall stone bottles, in which I had kept for some months

(b) M. BEAUME, in order to demonstrate the acid of vitriol in the clay, has boiled it for a considerable time with fixed alkaly, and thereby obtained tartar of vitriol: had he made use of a pure alkaly, I would take upon me to say, that his experiment would have failed. The alkaly he used contained already tartar of vitriol, which became more manifest by long boiling, as a portion of the alkaly combines with the clay.

(c) Might not this be substituted in the room of white lead for painters use, the white lead having many bad qualities, and being very injurious to some other colours?

oil

oil of tartar, *per deliquium*. This ware is made with tobacco-pipe clay and sand, and when well burnt is not acted on by either acids or alkalies.

This shews the union of clay with alkalies, and that may be the chief reason why it should, when helped with heat, be so useful in obtaining the acids of nitre and of sea salt. I have also obtained this gelatinous substance by mixture of tobacco-pipe clay and oil of tartar, *per deliquium*; for after some months, the alkaly being dissolved with water and evaporated, had in great part a gelatinous consistence. This mixture was stirred now and then, and had a remarkable volatile alkaline smell. Mr. BOYLE says, that clays distilled with sea salt produces a *sal ammoniac*, and I found it to be always true when the distillation is at first slowly conducted.

I have often made allum with tobacco-pipe clay calcined with oil of vitriol, but kept no notes of the quantity I obtained; and as for the *residuum* used in this experiment, it was lost. I can, however, tell the quantity of allum which the porcellane clay from Cornwall affords. Two drams of this clay, which had been treated with its weight of fixed alkaly, and deprived of its saline part, then calcined four different times, with a fresh portion each time of oil of vitriol, produced $\frac{3}{4}$ fl. and gr. xxiv. of good

good crystallized allum^(d). The fourth calcination afforded no allum, and what remained, after being dried and washed, weighed gr. lv. What this matter is I have not yet tried; perhaps it may be of the quartz kind. Hence it appears, that this clay contains half its weight of earth of allum, which, by its union with the acid of vitriol and the water that enters into its crystallization, produces better than four times its weight of allum, and therefore this clay treated with acid of vitriol affords more than double its weight of allum. .

Of Feld spar.

The honourable Mr. GREVILLE, a member of the Royal Society, and remarkable for his taste and skill in natural history, as well as for his judicious remarks on the nature, growth, and formation of minerals, has in his travels made some very interesting observations on the formation of Feld spar; the specimens which he collected on the spot shew evidently the change of clay into this spar, and also the different gradations of the change.

(d) The celebrated Mr. MARGRAF, to whom the discovery of making allum with *argilla* and acid of vitriol is due, could not obtain the allum in a crystallized state without the addition of some fixed alkaly. His mistake was owing to the excess of acid of vitriol, which the alluminous earth of the clay retained for want of sufficient heat, and which he corrected by saturating it with an alkaly.

The

The Feld spar I made use of was given me by that most excellent chemist, M. ROUELLE of Paris, and I think he told me it came from Alençon.

In order to make allum of it, I melted ℥ j. of it with ℥ j ff. of fixed alkaly of cream of tartar, and let the mixture run *per deliquium*. This operation is as tedious and as troublesome to perform as the *liquor silicum* made with flint, quartz, or crystal, for it is as liable to froth and boil over. No neutral salt was here obtained. The *deliquium* with its dregs were mixed with distilled vinegar, which precipitated the spar: this precipitate, after edulcoration and exsiccation, was calcined four different times, with fresh parcels each time of oil of vitriol, and afforded ℥ vij. and gr. xvi. of good and regularly crystallized allum; the part which afforded no more allum, when washed and dried, weighed ℥ j. and ℥ ij.

The Labradore stone ^(e) is also a Feld spar, though not so hard as the former. The like quantity of this, treated as the foregoing, gave no marks of neutral salt; the quantity of allum was ℥ j. ℥ j. and gr. xii.; and the un-

(e) This stone is only found on the Coast of Labradore, and was brought here by the direction of the Rev. Mr. LA TROBE, remarkable for his piety and zeal in propagating the gospel among the savage Indians. This stone reflects a variety of fine shining colours, such as blue, green, yellow, &c. I doubt not but that several other stones, which reflect various and changeable colours, upon trial will be found to be Feld spars.

diffolved part weighed ℥ v. and gr. xxxvi. This spar does not boil up in its fusion with the alkaly near so much as the other, and therefore the loss in the operation is trifling compared to it, and on that account it affords a greater proportion of allum.

Shirl frequently, though erroneously, called basalt.

The shirl I tried is of a brown colour, forms a mass of long minute prisms closely adhering together, and comes from Bohemia. Its brown colour is owing to iron. This shirl, treated in the same proportion and manner as the Feld spar, afforded no neutral salt. The quantity of allum was ℥ ij. ʒ j. and gr. xxiv.; but I must observe, that the two last crystallizations contained iron, and the mother water that remained was of an oily consistence, had a styptic taste, and resembled that obtained in making green copperas. The earthy matter here remaining weighed ℥ ij. and gr. lii; was light and of a grey colour. Hence it is evident, that shirl contains nearly as much earth of allum as the Cornish porcellane clay.

Mr. ILSEMAN, an ingenious apothecary and chemist at Clausthal in the Hartz, has assured me, that he obtained from pumice stone and shirl a *sal catharticus amarus*. He has, I presume, made his trials on shirl which

which is found with lava, and is the product of a volcano. I had none of it to try, but judge it, from its appearance, to be of a different nature from that found in mines.

Allum is commonly obtained from slate, which for that purpose is calcined for a considerable time. I know no substance so replete with it as the Irish slate, *lapis Hybernicus* of the druggists; for this, without any calcination, affords allum.

Allum is likewise obtained from the earth or clay of Solfatara, and from the red slate found near Saarbruck; but these have been exposed, no one can tell how long, to the heat of volcanos. Iron always accompanies allum, and thence the use of some alkaly, by which means the red chalk (*rubrica fabrilis*) is obtained.

Of jasper.

Many of the jaspers, so called, owe their origin to crystal or quartz coloured with iron; perhaps in a few instances to copper. These, I dare say, treated as the former, would produce no allum; but I have only tried one of the Oberstein jaspers, and this had visible marks of crystal. Others of the jaspers, and these I call the true ones, are formed by clay and afford allum; such is the

Saxon jasper, called ribbon agate, as also a red one, which was given me as coming from Johngeorgenstadt. From those I obtained very good allum, but can give no account of the particulars, having lost the paper which contained the result of my experiments.

Yellow pitch stone, and wood like dale, petrified with pitch stone, both from Hungary.

The result of my experiments on these substances was set down on the same paper with that of the jaspers which was lost; but I recollect to have only obtained a small portion of allum, and for that purpose was obliged, before I obtained the allum well crytallized, to wash away the excess of acid with rectified spirit of wine.

Tin spar of the Germans, commonly called white tin ore.

It has a sparry appearance; but by its lamellated texture and great specific gravity, which is equal to that of tin grains, is easily known. This is supposed by several to be rich in tin; but the Saxon mineralogists assert, that it contains none. The only experiment I made with it was to digest it in a powdered state with acids, by which means it acquires a rich yellow colour, like turbith mineral;

neral; the acid of salt answers best for this experiment. This is the only substance I know of which has this property.

Norway zoolite.

This substance, treated with fixed alkaly, as in the former experiments, afforded no neutral salt; and what remained, after the alkaly was washed away, treated with acids, formed the like gelatinous matter as it does in its crude state.

An account of such substances as have a sparry appearance, and how to distinguish the one from the other.

Cryстал and quartz are easily known by their great hardness, and by the copious sparks of fire which they afford when struck with steel.

Feld spar is frequently so hard as to strike fire with steel, and to give copious sparks of fire; but its laminated texture, as well as its breaking into rhombs, makes it easily distinguished from cryстал and quartz.

Phosphoric spar is easily known by the luminous appearance it has when heated, and also by the smell it

affords when added to oil of vitriol made hot, which exactly resembles that of acid of salt. It is found of various colours, as green, blue, purple, crimson, white, and also yellow. When crystallized it forms perfect cubes; the only exception I ever met with was a specimen of the green sort, which was sent me by Mr. SOPER, of St. Columb in Cornwall, who has distinguished himself by his skill in natural history and mineralogy. This specimen was composed of two quadrangular pyramids united together at their basis.

Selenitical spar is found crystallized in a great variety of forms; is heavier than the foregoing substance; does not effervesce or dissolve with acids, nor is it so hard as the phosphoric spar.

Calcareous spar is easily distinguished from other substances by its effervescences and solution in the acids of nitre and sea salt. Acid of vitriol, added to these solutions, causes a selenitical precipitation: this spar is neither so hard nor so heavy as the foregoing substances, and crystallizes in a great variety of forms.

Gypsum is lighter than any of the foregoing substances, and is so soft as to be easily scratched with one's nail; it does not effervesce nor dissolve with acids, and is the only substance that forms plaster when burnt; it crystallizes in a great variety of forms.

Mica or Glimmer. This is lighter than any of the sparry substances; is composed of very thin flexible flakes, more or less large. Many of this kind are found in form of small coloured scales of various colours, and very much resemble the bronzes.

Tin spar, or white tin ore, see its description heretofore.

White lead ore is found of a great variety of forms; is very heavy, effervesces with the acids of nitre and sea salt, and totally dissolves in them, with the help of heat. With the acid of salt it forms crystals much resembling a silver coloured glimmer, just as common lead would have done, and with the acid of nitre it forms regular crystals.

The spathose iron ores, when powdered and put on a red-hot iron or stone, instantly become black, and look like a black, shining, micaceous iron ore.

Zeolite is lighter than the calcareous spars: see its properties already described.

Of a set of spars whose properties were not hitherto known, and experiments made on one of them.

This spar crystallizes in form of flat, and also of solid, rhomboidal crystals, and is found of a great variety of colours, such as white and semi-transparent, of a pearl colour,

colour, reddish, and of different shades of brown and yellow, some of them being like gold, brass, and copper. It has always a peculiar gloss or brightness. May not the green and yellow glimmers from Johngeorgenstadt be of this kind?

The specimen I made use of for the following experiments is in Dr. HUNTER's collection, and seemed to be well adapted for this purpose, it being perfectly free from any matrix or heterogeneous matter. The whole substance of it is a crust composed of rhomboids, which grow out of one another, and form a variety of cavities: its colour is white and semi-pellucid, and on one side there was a slight marcasitical coating. This substance comes from Joachimsthal, and is harder than any of those mentioned before, except the crystal, quartz, and feld spar.

EXPERIMENT I.

Three drams of this spar, with an equal quantity of fixed alkaly of cream of tartar, melt with a moderate degree of tin; but the mixture soon becomes thick and cakes. It was after an hour's calcination suffered to cool, then was powdered and deprived of its saline part with boiling distilled water. This saline part, treated as in the
other

other experiments with distilled vinegar and rectified spirit of wine, gave no marks of neutral salt. The undissolved part after this operation dried, weighed 3 ij. and gr. l.: this was mixed with oil of vitriol, which caused a strong effervescence, and then calcined, to be deprived of its excess of acid. It was now digested, at three different times, with distilled water, which dissolved a portion of it; what remained undissolved after this operation, being dried, weighed 3 j. and gr. lii. and was a selenite, as will appear by further experiments. The three portions of water, with which this matter was digested, mixed together, then evaporated and crystallized, produced 3 ij. and gr. xlv. of a white salt, mostly consisting of rhomboidal prisms, some lying flat and some erected sideways. This I judged, by its styptic taste, to contain iron, and by its white colour to contain a *sal catbarticus amarus*, or at least a new earth, which with the acid of vitriol forms a soluble salt.

EXPERIMENT II.

This spar, in its crude state, effervesces strongly with oil of vitriol diluted with water. Three drams of the spar treated in this manner, and afterwards deprived by calcination of its excess of acid, and then of its saline part by distilled

distilled water, left a selenite, which when dried weighed ʒij. and gr. xx. This proportion is greater than in the former experiment, which I attribute to the acid of vitriol having been diluted with water, and its combining on that account in a greater quantity with the calcareous earth of the spar. The saline part by evaporation and crystallization produced ʒij. and gr. xxviii. of salt, composed of small long crystals, like Epsom salts; its taste was bitter and styptic.

This spar, in its natural state, effervesces strongly with rectified acid of salt, produces heat, and totally dissolves in it; the solution is of a fine yellow colour.

Rectified acid of nitre dissolves also this spar with effervescence and heat; but the solution is colourless.

I judged from the foregoing experiments that this spar was composed of calcareous earth, some iron, and a portion of some other earth, which, with acid of vitriol, forms a soluble salt.

EXPERIMENT III.

In order to determine the quantity of calcareous earth this spar contained, I dissolved three drams of it in rectified acid of salt, and by the addition of a sufficient quantity of a solution of tartar of vitriol in water, I obtained
a selenite,

a felenite, which, when washed and dried with a strong heat, weighed 3 iij. and gr. xxxvi.

Three drams of dried whiting, dissolved in the same acid, and treated in the like manner, afforded 3 iv. and gr. xviii. of felenite. The whiting was all dissolved, except three grains. The calcareous earth contained in the spar must, by its formation into felenite, be increased in weight in the same proportion as the whiting; and hence it is evident, that three drams of this spar contains 3 j ff. and gr. xix, of calcareous earth. The remaining part must be the earth above-mentioned, with some iron.

EXPERIMENT IV.

In order to judge of the quantity of iron that this spar contained (not having any more of it left) I took the salt of the first experiment, and dissolved it with water, to which I added some acid of salt. I then precipitated the iron in form of Prussian blue with the common alkaline *lixivium* used for that purpose, and thereby obtained sixteen grains of a fine deep-coloured blue. Acid of salt was added in the usual manner, to heighten the colour, by which means nothing but iron was precipitated.

Two drams of common green copperas, treated in the same manner, produced 3 j. less than two grains of the

same sort of blue, though not so good. Hence we may conclude, that the salt of the first experiment, which was produced by three drams of spar with acid of vitriol, contained thirteen grains of vitriol of iron.

Prussian blue, made without help of allum, contains nearly half its weight of iron. Hence, from the foregoing experiments, three drams of this spar contain gr. viii. of iron, 3j. and gr. xlix. of calcareous earth, and 3j. gr. iij. of the earth of *sal catharticus amarus*, or perhaps some other earth, which forms, with acid of vitriol, a soluble salt.

The spathose iron ore being frequently found crystallized like the foregoing new spar, and having also a gloss on it, I was willing to try whether it had any affinity with it; but by the following experiments it appears to be of a different nature, not containing any calcareous earth.

The spathose iron ore dissolves almost totally in acid of salt, and the solution is of a deep yellow colour. A solution of tartar of vitriol in water added to it causes no precipitation; and hence it is evident, that it contains no calcareous earth. Acid of vitriol treated with the spathose iron dissolves the whole of it, excepting a few dregs; another proof of its containing no calcareous earth. The acid of nitre dissolves also this spar, and the solution is colourless.



IV. *Account of a Petrefaction found on the Coast of East Lothian.* By Edward King, Esq. F. R. S.

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

John-street,
November 14, 1778.

Read Nov. 26, 1778. **I**N consequence of the honour you did me, to put into my hands a very curious specimen of a recent petrefaction, permit me now to offer to your consideration a few thoughts concerning this production, which have occurred to me on comparing it with others of a similar kind, and which may at least serve as hints for further investigation.

We should not venture, it is true, without great caution, to speculate on these matters, as hasty and specious conclusions may easily be drawn by any one who indulges too readily a quick and lively imagination, which will ever be too ready to mislead, rather than to procure solid information. But though I

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am

am well aware of this danger, yet I venture to lay before you these few observations; and if you judge them at all worthy of attention, I would wish to communicate them, through your hands, to the Royal Society, for they are not made merely in consequence of a flight and hasty survey of this one specimen, but are in truth conclusions that I have been led to form incidentally in the course of a very long inquiry, which I have been for some years pursuing on another occasion; the result whereof I shall perhaps, if I live, hereafter communicate to the Society in a more full and explicit manner than the compass of a paper of this kind will permit.

The account of this specimen, with which you favoured me, is as follows. In the year 1745, the Foxman of war was unfortunately stranded on the coast of East Lothian in Scotland, and there went to pieces; and the wreck remained about three and thirty years under water; but this last year a violent storm from the North-east laid a part of it bare, and several masses, consisting of iron, ropes, and balls, were found on the sands near the place, covered over with a very hard ochry substance, of the colour of iron, which adhered thereto so strongly, that it required great force to detach it from the fragments of the wreck. And, upon examination, this substance

stance appeared to be sand, concreted and hardened into a kind of stone.

The specimen now laid before the Society had been taken out of the sea, from the same spot, some time before, and is a consolidated mass that had undergone the same change. It contains a piece of rope that was adjoining to some iron ring, and probably had been tied thereto. The substance of the rope is very little altered; but the sand is so concreted round it, as to be as hard as a bit of rock, and retains very perfectly impressions of parts of the ring, just in the same manner as impressions of extraneous fossil bodies are often found in various kinds of strata.

Now, considering these circumstances, we may fairly conclude, in the first place, that there is, on the coasts of this island, a continual progressive induration of masses of sand and other matter at the bottom of the ocean, somewhat in the same manner as there is at the bottom of the Adriatic sea, according to the account given by Dr. DONATI^(a).

And, in the next place (which is what more particularly deserves our attention on this occasion), it should seem, that iron, and the solutions of iron, contribute very much to hasten and promote the progress of the concretion and induration of stone, whenever they meet and are

(a) See the *Phil. Trans.* vol. XLIX. p. 588.

united with those cementing cryſtalline particles, which there is reaſon to believe are the more immediate cauſe of the conſolidation of all ſtones and marbles whatſoever, and which do very much abound in ſea water.

It would exceed the limits of this paper, were I to attempt here to mention only a ſmall part of the various facts that have come to my knowledge; and which have convinced me, and I truſt, when offered fairly to the conſideration of others, will make it appear fully to all that are attentive to theſe kind of reſearches, that there is, to this very day, a formation of ſtone, and even of marble itſelf, in certain places, in a much more perfect manner, than has been generally conceived; and far beyond what has been ſuppoſed, even by thoſe who have been ready too haſtily to account for ſuch a proceſs merely in conſequence of obſervations made on ſtalactitical and ſuch like ordinary concretions.

I ſhall not therefore preſume to trouble the Society, at preſent, with any detail of the inquiries I have made relating to that ſubject, though in reality they have been the foundation of the obſervations made in this paper; but ſhall confine my remarks merely to this one curious circumſtance; that wherever there is any induration and petrefaction of matter, from any cauſes whatever, it is greatly haſtened in its progreſs, and the conſolidation is rendered much more complete and firm by
being

being near any mass of iron, and still more so by the admixture of any solution of that metal.

This appears, in some degree, from the present specimen; where, near adjoining to the ring, and in the portion of the fragment that has the largest impression thereof, the concreted sand-stone is of a firmer texture, and there is a larger cohering mass formed about that part of the rope, than about those parts that are further removed from the ring.

It appears also from a circumstance that was particularly taken notice of when the wreck was discovered by the storm this year (and which is mentioned, SIR, in the letter you favoured me with a sight of); for the masses that were observed to have these concretions adhering to them, were not masses of timber, or other large fragments of the wreck, which one would think, on a slight consideration of the matter, were most likely to cause obstructions at the bottom of the ocean, and to form little banks of sand, that might afterwards be concreted; but they were masses of loose iron and ropes, and even of cannon balls, which were thus consolidated.

The same conclusion also may be drawn, with still more appearance of its being well founded, from a very remarkable piece of antiquity, which was discovered about three years ago on the coast of Kent. Some fishermen, sweeping for anchors in the Gull stream (a part of

the sea near the Downs), drew up a very curious old swivel gun, near eight feet in length. The barrel of the gun, which was about five feet long, was of brass; but the handle (whereby it was to be turned or traversed) which was about three feet in length, and also the swivel and pivot on which it turned, were of iron, and all round these latter, and especially about the swivel and pivot, were formed exceeding hard incrustations of sand, converted into a kind of stone, of an exceeding strong texture and firmness; whereas round the barrel of the gun, except where it was near adjoining to the iron, there were no such incrustations at all, the greater part of it being clean, and in good condition, just as if it had still continued in use^(b).

The incrustation round the iron part of this gun was also the more deserving of attention, because it inclosed within it, and also held fastly adhering to it on the outside, a number of shells and corallines, just in the same manner as they are often found in a fossil state. There were plainly to be distinguished, on the outside of this mass of incrustation, pectens, cockles, limpets, muscles, *vermiculi marini* and *balini*; and besides these, one *bucci-*

(b) As there were several remarkable particularities in this gun, tending to explain some curious facts in history, I took the liberty to give a full account of it, with a view to illustrate them, in a memoir laid before the Antiquarian Society last year.

mm^(c) and one oyſter; and they were all ſo thoroughly and ſtrongly fixed thereto, and themſelves alſo converted into ſuch an hard ſubſtance, that it required as much force to ſeparate or break them, as to break a fragment off any hard rock; and in colour and appearance, they much reſembled ſome of the maſſes of foſſil bodies found near Chippenham in Wiltſhire.

This remarkable incruſtation, therefore, thus confined to the parts of the gun adjoining to the iron, and appearing no where elſe upon it, plainly indicates, that the iron was, by ſome means or other, the more immediate cauſe thereof: and yet it is to be obſerved, that in this inſtance the iron was very little diſſolved; for although it is manifeſt, from ſome circumſtances in its hiſtory, that the gun muſt have remained in the ſea above two hundred years, and probably a great deal longer, yet the greater part of the handle and of the ſwivel remained entire, and even the point of the pivot was undiſſolved, and very viſible.

Another curious appearance alſo, of a ſimilar nature, will tend further to confirm the obſervations here made. This is found in a ſpecimen (now in my poſſeſſion) of a moſt remarkable incruſtation, that was formed in the ſpace of three years only, within a ſquare wooden pipe, in a Coal Mine in Somerſetſhire. I gave a very particu-

(c) See the figure marked G 41.

lar account of this production in a paper laid before the Society in the year 1773^(d), and therefore shall say nothing of it here but what immediately relates to the present purpose. The pipe was forty-two feet in length, and the hollow part of it was seven inches and an half wide, by four inches and an half; and this whole cavity was so filled with the incrusted matter (which was hard enough to take an exceeding fine polish, like the most beautiful marble) that at last there was left a water way, which, nearly uniformly throughout the whole length of the pipe, was only about three inches and an half by one inch; and thus there was formed, within the first wooden pipe, a second pipe of this incrustation, the thickness of the sides of which was about one inch and an half. On cutting a transverse section of this pipe there appeared a number of uniform lines, forming almost regular similar parallelograms, one within another, like the coats of an onion, and plainly denoting the gradual and regular progress of the formation of the whole incrustation. But the circumstance most remarkable, and that is more immediately applicable to the present purpose, is, that where there was, by accident, the point of a nail projecting through the side of the wooden pipe, it so accelerated the progress of the incrustation, that, adjoining thereto, the similar sides of the first and outward

(d) Phil. Trans. vol. LXIII. p. 241.

parallelograms next the wooden pipe (instead of continuing as straight lines) formed in that place very bold semi-circular curves, or protuberances, one beyond another, projecting from the wooden pipe inwards; and this curvilinear projection was uniformly continued throughout all the similar parallelograms quite to the inward cavity of the pipe formed by the incrustation, and there at last occasioned a projecting gibbosity, of a considerable extent every way, from the point of the nail^(e).

Having mentioned these remarkable facts to my very learned and ingenious friend Dr. FOTHERGILL, I had the pleasure to find they struck him much in the same light in which they had appeared to me; and that he, moreover, formed the same conclusion concerning the specimen now laid before the Society that you had also formed and mentioned in the note you favoured me with when you sent the specimen to my house; namely, that the concretion was effected by the solution of the adjoining iron ring.

Dr. FOTHERGILL also (who had communicated some very original conjectures upon this subject to the Society many years^(f) ago) informed me of some further

(e) There is another fragment of the incrustation formed within this pipe, with a transverse section thereof polished, in the Museum of the Royal Society; but that with the point of the nail, which is here alluded to, is still in my possession.

(f) In a paper on the Origin of Amber, read in the year 1743.

curious facts, which he permits me now to mention in this paper.

On passing through the streets of London in his walks, before the sign-irons were taken down, he perceived, that on the broad stone pavements, whenever he came just under any sign-irons, his cane gave a different sound, and occasioned a different kind of resistance to the hand, from what it did elsewhere; and attending more particularly to this circumstance, he found, that every where, under the drip of those irons, the stones had acquired a greater degree of solidity, and a wonderful hardness, so as to resist any ordinary tool, and gave, when struck upon, a metallic sound: and this fact, by repeated observations, he was at length most thoroughly convinced of.

Taking the hint, therefore, from hence, he thought fit to make several experiments; and, amongst the rest, placed two pieces of Portland stone in the same aspect and situation in every respect, but washed the one frequently with water impregnated with rusty iron, and left the other untouched: and in a very few years he found the former had acquired a very sensible degree of that hardness before described, and on being struck gave the metallic sound; whilst the other remained in its original state, and subject to the decays occasioned by the changes
of

of the weather, which we find in many instances make a most rapid progress.

He also mentioned to me a very curious circumstance of his having found on the sea-coast near Scarborough, many years ago, part of an horse-shoe incruusted with sea sand, which was so concreted as to have acquired the hardness of common grit stone, and retained the colour of the sand, with very little tincture of the iron ochre: and by the part which was left free from the incrustation, it appeared most probable, that the horse-shoe had not been buried there many years, but had very recently acquired this incrustation upon that part only that was most exposed to the washing of the sea water.

Now, all these facts put together scarce leave any doubt but that iron, and solutions of iron, do greatly promote and hasten the progress of all kinds of petrification; and therefore, to pursue Lord BACON's rule of induction, deducing truth from a variety of facts and experiments, all manifestly and uniformly leading us to the same conclusion, we may fairly infer, that whereas iron is of such manifest use in the progress of vegetation, that plants are indebted for their green colour^(g), and for

(g) There are some exceeding curious experiments and observations of Mr. DELAVAL's, tending to illustrate this fact, in the Phil. Trans. vol. LV. p. 36, 37.

many of their valuable qualities, to its being intimately mixed in their substance; and as it is moreover obviously useful, some way or other, in the animal system, and may be extracted by the magnet from the ashes of animal substances; so it is no less useful in the consolidation of stones and marble in the fossil world.

Mr. PRYCE, in a very useful and curious treatise of Mineralogy^(b), has moreover lately shewn it to be equally useful in the mineral world, by forming a proper *nidus* for the assemblage of the most valuable metals, and attracting and uniting them thereby. This metal, therefore, seems to be almost universally one of the greatest bands that unites the several parts of matter, and one of the most useful and important of substances in the world.

It is not for us to presume to comprehend any thing about the original formation of bodies. Such disquisitions are far out of the reach of our faculties; nor do I at all pretend to enter into them: but we are permitted to behold and consider the works of the Almighty, and may become wiser and reap profit from the contemplation of them, and may perceive in what manner many new combinations of matter are continually effected.

And as we manifestly perceive plants to grow daily for the necessary supplies of life, without knowing how

(b) *Mineralogia Cornubiensis*, p. 6. 11. 67.

they

they grow; but yet are convinced they are nourished by means of the salts and particles of matter conveyed to them by water and vapour, and that iron is a necessary ingredient in their composition; so I am persuaded we shall, at length, perceive (notwithstanding the general opinion to the contrary) that even stones and marbles are still continually forming in the earth for the services of human life, and to supply the continual waste and decay that there is of those substances; and that the consolidation of them is effected by means of water and vapour likewise (of which I am able to produce very many and convincing proofs); and that iron, which is what alone I wish now to make apparent, is unquestionably a principal means of effecting the induration of many of those bodies.

But I would not willingly trouble the Society with this paper as a matter of mere speculation. I would wish that some hint, which might be more immediately useful, should, if possible, be derived therefrom; and such has been suggested to me by Dr. FOTHERGILL.

If iron and the solutions of iron do thus contribute to the induration of bodies; such solutions must probably have that tendency in every stage of those bodies' existence; and therefore it seems likely, that the fine ornamental carvings in Portland, or other stone, might be much

much hardened, and preserved for a much longer time than has been usual, from the injuries of the weather, by being washed and brushed over by water, in which is infused a solution of iron. And perhaps even the softer kinds of stones might have been preserved by this means; and the venerable remains of that fine pile of building HENRY the VIITH's chapel, might have been saved from the destruction with which we now see it ready to be overwhelmed.

It is very probable, moreover, that common sea sand, with a very small admixture of a solution of iron, may at length, without any great expence, be converted into a most useful species of stone, and be applied to the purpose of covering the fronts of houses even more durably, and in as beautiful a manner as some of the late invented stuccos; and even those stuccos may be improved by means of the same mixture.

It must be left to future experiments to ascertain what proportions of the solutions of iron are best to be made use of; and in what manner the solution may best be obtained for this purpose; whether by a vitriolic acid; or merely by laying rusty iron for some time in water⁽ⁱ⁾. But one caution should be observed; namely, that if too

(i) It is not perhaps possible to contrive a less saturated tincture of iron than the rust dissolved in water.

great

great a proportion of vitriolic acid be left in the mixture, it may do more mischief than the iron can do good. With this caution it were perhaps much to be wished, that experiments^(b) should be made, and attended to, for a long course of years; for without such continued and repeated trials, it will be impossible to determine in what manner the application may be made with most advantage.

I have only therefore to add, that what has been said may perhaps receive some further confirmation, from considering that the chief constituent parts of all cements for stone are always lime and iron⁽ⁱ⁾; and that any experiments made in consequence of these observations will be likely to succeed more rapidly in warm climates than in colder ones.

Whatever there is of inaccuracy in this short paper will I am persuaded meet with indulgence both from you and from the Society, whilst whatever truth is brought to light thereby will be received and adopted. I therefore venture to deliver it into your hands without hesitation. I am, &c.

(b) The ages of modern buildings are easily known, and there are none but what have iron rails or bars about them exposed to the weather. If the rain drops from them on Portland or any gritty stone, it becomes harder. The length of time such stones have been subjected to this process may easily be known, and the effects produced in them may be verified by repeated comparisons of different fragments.

(i) See CROUSTEDST'S Mineralogy, p. 45.

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I have added a few rough sketches to explain what has been said in this letter.

FIG. I. Represents the mass of concreted sand and rope from the coast of Lothian.

AB shews where there is one impression of a part of the iron ring.

CD shews where there is also another impression of the ring; and

EF shews where there also remain some impressions of bits of untwisted rope, so that this whole mass seems to have been formed within the circumference of the ring.

FIG. II. Represents a transverse section of the petrefaction within the wooden pipe, from the coal mine in Somersetshire; and

G shews the place of the point of the nail, and the regular curvilinear projection of the petrefaction around it, regularly throughout the whole progress.

FIG. III. Is a sketch of the gun, shewing the parts to which the incrustation adhered.



V. *Account of Dr. Knight's Method of making artificial Loadstones.* By Mr. Benjamin Wilson, F. R. S.

TO JOSEPH BANKS, ESQ. P. R. S.

§ I R,

Read Dec. 17,
1778.

THE method of making artificial loadstones, as it was discovered and practiced by the late Dr. GOWIN KNIGHT, being unknown to the public; and I myself having been frequently present when the doctor was employed in the most material steps of that curious process, I thought a communication thereof would be agreeable to you and the philosophic world.

The method was this: having provided himself with a large quantity of clean filings of iron, he put them into a large tub that was more than one-third filled with clean water: he then, with great labour, worked the tub to and fro for many hours together, that the friction between the grains of iron by this treatment might break off such

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smaller

smaller parts as would remain suspended in the water for a time. The obtaining of those very small particles in sufficient quantity seemed to him to be one of the principal *desiderata* in the experiment.

The water being by this treatment rendered very muddy, he poured the same into a clean earthen vessel, leaving the filings behind; and when the water had stood long enough to become clear, he poured it out carefully, without disturbing such of the iron sediment as still remained, which now appeared reduced almost to impalpable powder. This powder was afterwards removed into another vessel, in order to dry it; but as he had not obtained a proper quantity thereof, by this one step he was obliged to repeat the process many times.

Having at last procured enough of this very fine powder, the next thing to be done was to make a paste of it, and that with some vehicle which would contain a considerable quantity of the phlogistic principle; for this purpose he had recourse to linseed oil in preference to all other fluids.

With these two ingredients only he made a stiff paste, and took particular care to knead it well before he moulded it into convenient shapes. Sometimes, whilst the paste continued in its soft state, he would put the impression

impreſſion of a ſeal upon the ſeveral pieces; one of which is in the Britiſh Muſeum.

This paſte was then put upon wood, and ſometimes on tiles, in order to bake or dry it before a moderate fire, at a foot diſtance or thereabouts.

The doctor found, that a moderate fire was moſt proper, becauſe a greater degree of heat made the compoſition frequently crack in many places.

The time required for the baking or drying of this paſte was generally five or ſix hours before it attained a ſufficient degree of hardneſs. When that was done, and the ſeveral baked pieces were become cold, he gave them their magnetic virtue in any direction he pleaſed, by placing them between the extreme ends of his large magazine of artificial magnets for a few ſeconds or more, as he ſaw occaſion.

By this method the virtue they acquired was ſuch, that when any one of thoſe pieces was held between two of his beſt ten guinea bars, with its poles purpoſely inverted, it immediately of itſelf turned about to recover its natural direction, which the force of thoſe very powerful bars was not ſufficient to counteract.

I am, &c.



VI. *Account of an extraordinary Dropsical Case. By Mr. John Latham, in a Letter to Mr. Warner, F. R. S.*

T O ~~MR.~~ W A R N E R.

S I R,

Dartford,
October 28, 1778.

Read Dec. 17, 1778. **W**HEN I last had the pleasure of seeing you, it was your opinion, that the Royal Society would receive some satisfaction in my giving some account of the case of Miss A. M. who died lately of a dropfy under my care.

This patient was of a florid, lively constitution, but from a child was subject to a violent eruption, which came generally on a sudden, covering the whole neck, breast, and often great part of the face; and after remaining a week or two abated in violence, and went off by degrees. The intervals were uncertain, but for the most part in spring and in autumn she was more apt to have it, though frequently three or four times in the year. Various methods were tried to eradicate this complaint

plaint without effect; nor did the appearance of the menses, as we had some reason to hope, in the least turn out in her favour. It will be needless to relate here the various medicines which had been given her with little or no success, except that the most relief she found was from the use of salt water, which was thought to make the intervals the longer in two or three instances, as well as the appearance of the eruption milder. Things continued thus till the autumn 1773, when the menses became obstructed, continuing so for some months, but appeared once more very plentifully; after which they never returned, neither did the eruption, except in the most trifling manner. About Christmas 1773 she complained of a weight in the abdomen, and fulness of the stomach; which symptoms were relieved by some gentle opening medicines. She then went on a visit to some friends at a distance, after which I saw her no more for two months. I learned, that during that time the complaints had returned more violent, for which she consulted a physician on the spot, but without the relief she found at first; for the abdomen began to increase in size every day, and became painful, the urine high-coloured, and in small quantity, with thirst, and every other symptom of an approaching dropfy.

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In a narrative of this kind it may be expected, that a detail of the medicines she took during her illness might be noted; but as I chiefly acted in my surgical capacity, and as she was after this time, till the first operation, for the most part in London, under the care of physicians of the first eminence, it is out of my power to give such an account; suffice it then to say, that she was obliged to submit to the operation of the *paracentesis* the 27th of June, 1774. The quantity I then took off was only twelve pints, somewhat foetid, but not very dark coloured, nor was it ever after the least offensive. The operation was repeated in six weeks, when twenty-nine pints were taken off; after that time once in four weeks to the end of the year. During the whole of the year 1775 I tapped her once in a fortnight more or less; and in the year 1776 she for the most part underwent the operation every eight or nine days, the intervals gradually shortening, till by the end of the year she could go no longer than a week between, which continued to the day of her death, which happened May 14, 1778, being then not quite twenty-three years of age. About a week before that time, she was troubled with incessant vomitings, which nothing would relieve; but was better a few hours before her death, and went off pretty easy.

I have good reason to suppose the complaint originated from a disease of the left ovary, for after the first tapping I felt a substance of the size of a cricket ball; and, as the operations went on, this became more and more manifest, increasing so much as at last to occupy the whole space of the *abdomen* forward, being of a very irregular form, and I am clear of many pounds weight, for she appeared, even after the water had been drawn off, as large as a woman in the last month of pregnancy. It would have added greatly to my satisfaction to have been able to clear up this point in every particular, by opening her after death; but I had the extreme mortification of being denied this necessary circumstance, notwithstanding my most earnest solicitations.

I must, therefore, content myself with giving this bare recital of facts as above, which will serve to record to futurity, a case which I believe has not its equal in regard to the number of operations. What is remarkable here is, that this young lady had a good appetite for the most part, and was very chearful; and, except a day before and after each operation, used to visit her friends at several miles distance as she would have done in health, and till within the last two or three months could walk a mile or two with tolerable ease.

As to the quantity of water taken off, I find it to amount, upon the nearest calculation, to twenty-four pints at each operation; for though the first time produced only twelve pints, and in several of the latter operations the quantity fell short of twenty-four pints, yet I may venture to state it at least at twenty-four pints or three gallons on an average, as in many of the operations I took off from twenty-eight to thirty pints. The number of times I tapped her was in all 155, which brings out in the whole 3720 pints, being 465 gallons, not far short of seven hogheads and an half. As to the authenticity of the whole, your connections with the family, and frequent opportunities of seeing this young lady during her illness, will put it beyond a doubt. I have therefore no more to add, than my wish that the case may prove acceptable to the Society.

I am, &c.

VII. *Problems concerning Interpolations.* By Edward Waring, M. D. F. R. S. and of the Institute of Bononia, Lucasian Professor of Mathematics in the University of Cambridge.

Read Jan. 9, 1779. **M**R. BRIGGS was the first person, I believe, that invented a method of differences for interpolating logarithms at small intervals from each other: his principles were followed by REGINALD and MOVTON in France. SIR ISAAC NEWTON, from the same principles, discovered a general and elegant solution of the abovementioned problem: perhaps a still more elegant one on some accounts has been since discovered by Mess. NICHOLE and STIRLING. In the following theorems the same problem is resolved and rendered somewhat more general, without having any recourse to finding the successive differences.

T H E O R E M I.

Assume an equation $a + bx + cx^2 + dx^3 \dots x^{n-1} = y$, in which the co-efficients a, b, c, d, e , &c. are invariable;

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let

let $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ denote n values of the unknown quantity x , whose correspondent values of y let be represented by $s^\alpha, s^\beta, s^\gamma, s^\delta, s^\epsilon, \&c.$ Then will the equation $a + b x + c x^2 + d x^3 + e x^4 \dots x^{n-1} = y =$

$$\frac{\overline{\beta \times \gamma \times \delta \times \epsilon \times \&c.} - \overline{\delta \times \gamma \times \epsilon \times \&c.} \times \&c.}{\alpha - \beta \times \alpha - \gamma \times \alpha - \delta \times \alpha - \epsilon \times \alpha - \&c.} \times S^\alpha + \frac{\overline{\delta \times \gamma \times \epsilon \times \&c.} - \overline{\epsilon \times \gamma \times \&c.} \times \&c.}{\beta - \alpha \times \beta - \gamma \times \beta - \delta \times \beta - \epsilon \times \beta - \&c.} \times S^\beta \\ + \frac{\overline{\epsilon \times \gamma \times \&c.} - \overline{\&c.} \times \&c.}{\gamma - \alpha \times \gamma - \beta \times \gamma - \delta \times \gamma - \epsilon \times \gamma - \&c.} \times S^\gamma + \frac{\overline{\&c.} - \overline{\&c.} \times \&c.}{\delta - \alpha \times \delta - \beta \times \delta - \gamma \times \delta - \epsilon \times \delta - \&c.} \times S^\delta \\ + \frac{\overline{\&c.} - \overline{\&c.} \times \&c.}{\epsilon - \alpha \times \epsilon - \beta \times \epsilon - \gamma \times \epsilon - \delta \times \epsilon - \&c.} \times S^\epsilon + \&c.$$

DEMONSTRATION.

Write α for x in the equation $y =$

$$\frac{\overline{\beta \times \gamma \times \delta \times \epsilon \times \&c.} - \overline{\delta \times \gamma \times \epsilon \times \&c.} \times \&c.}{\alpha - \beta \times \alpha - \gamma \times \alpha - \delta \times \alpha - \epsilon \times \alpha - \&c.} \times S^\alpha + \frac{\overline{\delta \times \gamma \times \epsilon \times \&c.} - \overline{\epsilon \times \gamma \times \&c.} \times \&c.}{\beta - \alpha \times \beta - \gamma \times \beta - \delta \times \beta - \epsilon \times \beta - \&c.} \times S^\beta +$$

$\&c.$; and all the terms but the first in the resulting equation will vanish, for each of them contains in its numerator a factor $x - \alpha = \alpha - \alpha = 0$; and the equation will become

$y = \frac{\overline{\beta \times \gamma \times \delta \times \epsilon \times \&c.} - \overline{\delta \times \gamma \times \epsilon \times \&c.} \times \&c.}{\alpha - \beta \times \alpha - \gamma \times \alpha - \delta \times \alpha - \epsilon \times \alpha - \&c.} \times S^\alpha = S^\alpha.$ In the same manner, by writing $\beta, \gamma, \delta, \epsilon, \&c.$ successively for x in the given equation it may be proved, that when x is equal to $\beta, \gamma, \delta, \epsilon, \&c.$ then will y become respectively $s^\beta, s^\gamma, s^\delta, s^\epsilon, \&c.$ which was to be demonstrated.

2. Assume $y = ax^r + bx^{r+1} + cx^{r+2} + dx^{r+3} \dots x^{r+n-1}$; and when x becomes $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ let y become respectively

spectively $s^a, s^b, s^c, s^d, s^e, \&c.$; then will $y =$

$$\begin{aligned} & \frac{x^r \times x^i - \beta^r \times x^i - \gamma^r \times x^i - \delta^r \times x^i - \epsilon^r \times \&c.}{a^r \times a^i - \beta^r \times a^i - \gamma^r \times a^i - \delta^r \times a^i - \epsilon^r \times \&c.} \times s^a \\ & + \frac{x^r \times x^i - a^r \times x^i - \gamma^r \times x^i - \delta^r \times x^i - \epsilon^r \times \&c.}{\beta^r \times \beta^i - a^r \times \beta^i - \gamma^r \times \beta^i - \delta^r \times \beta^i - \epsilon^r \times \&c.} \times s^b \\ & + \frac{x^r \times x^i - a^r \times x^i - \beta^r \times x^i - \delta^r \times x^i - \epsilon^r \times \&c.}{\gamma^r \times \gamma^i - a^r \times \gamma^i - \beta^r \times \gamma^i - \delta^r \times \gamma^i - \epsilon^r \times \&c.} \times s^c + \&c. \end{aligned}$$

This may be demonſtrated in the ſame manner as the preceding theorem, by writing $a, \beta, \gamma, \delta, \epsilon, \&c.$ ſucceſſively for x .

PROBLEM.

Let there be n values $a, \beta, \gamma, \delta, \epsilon, \&c.$ of the quantity x , to which the n values $s^a, s^b, s^c, s^d, s^e, \&c.$ of the quantity y correſpond; ſuppoſe theſe quantities to be found by any function X of the quantity x ; let $\pi, \rho, \sigma, \tau, \&c.$ be values of the quantities x , to which $s^\pi, s^\rho, s^\sigma, s^\tau, \&c.$ values of the quantity y correſpond: for x ſubſtitute its abovementioned values $\pi, \rho, \sigma, \tau, \&c.$ in the function X , and let the quantities reſulting be $s^\pi, s^\rho, s^\sigma, s^\tau, \&c.$ not equal to the preceding $s^a, s^b, s^c, s^d, \&c.$ reſpectively; to find a quantity which added to the function X ſhall not only give the true values of the quantity y correſponding to the values $a, \beta, \gamma, \delta, \epsilon, \&c.$ of the quantity x , but alſo

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corresponding to the values π, ρ, σ, τ , &c. of the above-mentioned quantity x .

Assume $s''-s'=T''$, $s'-s'=T'$, $s'-s'=T''$, $s'-s'=T'$, &c.; then the errors of the function X will be respectively T'' , T' , T'' , T' , &c.; and the correcting quantity sought may be

$$\begin{aligned} & \frac{\overline{x-\alpha} \times \overline{x-\beta} \times \overline{x-\gamma} \times \overline{x-\delta} \times \overline{x-\epsilon} \times \&c.}{\overline{\pi-\alpha} \times \overline{\pi-\beta} \times \overline{\pi-\gamma} \times \overline{\pi-\delta} \times \overline{\pi-\epsilon} \times \&c.} \times \frac{\overline{x-\rho} \times \overline{x-\sigma} \times \overline{x-\tau} \times \&c.}{\overline{\pi-\rho} \times \overline{\pi-\sigma} \times \overline{\pi-\tau} \times \&c.} \times T'' \\ & + \frac{\overline{x-\alpha} \times \overline{x-\beta} \times \overline{x-\gamma} \times \overline{x-\delta} \times \overline{x-\epsilon} \times \&c.}{\overline{\rho-\alpha} \times \overline{\rho-\beta} \times \overline{\rho-\gamma} \times \overline{\rho-\delta} \times \overline{\rho-\epsilon} \times \&c.} \times \frac{\overline{x-\pi} \times \overline{x-\sigma} \times \overline{x-\tau} \times \&c.}{\overline{\rho-\pi} \times \overline{\rho-\sigma} \times \overline{\rho-\tau} \times \&c.} \times T' \\ & + \frac{\overline{x-\alpha} \times \overline{x-\beta} \times \overline{x-\gamma} \times \overline{x-\delta} \times \overline{x-\epsilon} \times \&c.}{\overline{\sigma-\alpha} \times \overline{\sigma-\beta} \times \overline{\sigma-\gamma} \times \overline{\sigma-\delta} \times \overline{\sigma-\epsilon} \times \&c.} \times \frac{\overline{x-\pi} \times \overline{x-\rho} \times \overline{x-\tau} \times \&c.}{\overline{\sigma-\pi} \times \overline{\sigma-\rho} \times \overline{\sigma-\tau} \times \&c.} \times T'' \\ & + \frac{\overline{x-\alpha} \times \overline{x-\beta} \times \overline{x-\gamma} \times \overline{x-\delta} \times \overline{x-\epsilon} \times \&c.}{\overline{\tau-\alpha} \times \overline{\tau-\beta} \times \overline{\tau-\gamma} \times \overline{\tau-\delta} \times \overline{\tau-\epsilon} \times \&c.} \times \frac{\overline{x-\pi} \times \overline{x-\rho} \times \overline{x-\sigma} \times \&c.}{\overline{\tau-\pi} \times \overline{\tau-\rho} \times \overline{\tau-\sigma} \times \&c.} \times T' \\ & + \&c. \end{aligned}$$

Aliter.

Let $\overline{x-\alpha} \times \overline{x-\beta} \times \overline{x-\gamma} \times \overline{x-\delta} \times \overline{x-\epsilon} \times \&c. \times \overline{x-\pi}$
 $\times \overline{x-\rho} \times \overline{x-\sigma} \times \overline{x-\tau} \times \&c. = N$; $\overline{\pi-\alpha} \times \overline{\pi-\beta} \times \overline{\pi-\gamma} \times \overline{\pi-\delta} \times \overline{\pi-\epsilon}$
 $\times \&c. \times \overline{\pi-\rho} \times \overline{\pi-\sigma} \times \overline{\pi-\tau} \times \&c. = \Pi$; $\overline{\rho-\alpha} \times \overline{\rho-\beta} \times \overline{\rho-\gamma} \times \overline{\rho-\delta} \times$
 $\overline{\rho-\epsilon} \times \&c. \times \overline{\rho-\pi} \times \overline{\rho-\sigma} \times \overline{\rho-\tau} \times \&c. = P$; $\overline{\sigma-\alpha} \times \overline{\sigma-\beta} \times \overline{\sigma-\gamma} \times$
 $\overline{\sigma-\delta} \times \overline{\sigma-\epsilon} \times \&c. \times \overline{\sigma-\pi} \times \overline{\sigma-\rho} \times \overline{\sigma-\tau} \times \&c. = \Sigma$; $\overline{\tau-\alpha} \times \overline{\tau-\beta} \times$
 $\overline{\tau-\gamma} \times \overline{\tau-\delta} \times \overline{\tau-\epsilon} \times \&c. \times \overline{\tau-\pi} \times \overline{\tau-\rho} \times \overline{\tau-\sigma} \times \&c. = T, \&c.$; then
 may the correcting quantity sought be $N \left(\frac{T''}{\Pi \times x - \pi} + \frac{T'}{P \times x - \rho} \right.$
 $\left. + \frac{T''}{\Sigma \times x - \sigma} + \frac{T'}{T \times x - \tau} + \&c. \right)$.

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This problem may be demonstrated in the same manner as the preceding theorems, by writing for x in the correcting quantity successively its values $\pi, \rho, \sigma, \tau, \&c.$

2. For the correcting quantity sought may be assumed the quantity

$$\frac{\frac{x' - a' \times x' - \beta' \times x' - \gamma' \times x' - \delta' \times \&c. \times x' \times x' - \epsilon' \times x' - \sigma'}{x' - a' \times x' - \beta' \times x' - \gamma' \times x' - \delta' \times \&c. \times x' \times x' - \epsilon' \times x' - \sigma'} \times \frac{x' - \tau' \times \&c.}{x' - \tau' \times \&c.} \times T^{\pi} + \frac{\frac{x' - a' \times x' - \beta' \times x' - \gamma' \times x' - \delta' \times \&c. \times x' \times x' - \epsilon'}{\epsilon' - a' \times \epsilon' - \beta' \times \epsilon' - \gamma' \times \epsilon' - \delta' \times \&c. \times \epsilon' \times \epsilon' - \sigma'} \times \frac{x' - \tau' \times \&c.}{\epsilon' - \sigma' \times \epsilon' - \tau' \times \&c.} \times T^{\rho} + \&c.$$

3. In general, let z be any quantity which is $=0$, when x becomes either $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$: let z become successively $A, B, C, D, \&c.$ when x becomes $\pi, \rho, \sigma, \tau, \&c.$ respectively. When x either $= \rho, \sigma, \tau, \&c.$ let $\Pi = 0$; but if $x = \pi$, let $\Pi = p$: in the same manner when x either $= \pi, \sigma, \tau, \&c.$ let $P = 0$; but when $x = \rho$ let $P = r$: and similarly, let $\Sigma = 0$ when x is either $\pi, \rho, \tau, \&c.$; but when $x = \sigma$ let $\Sigma = s$: and likewise, when x is either $\pi, \rho, \sigma, \&c.$ let $T = 0$; but when $x = \tau$ let $T = t$; $\&c.$ then for the correcting quantity sought may be assumed $\frac{z}{A} \times \frac{\Pi}{p} \times T^{\pi} + \frac{z}{B} \times \frac{P}{r} \times T^{\rho} + \frac{z}{C} \times \frac{\Sigma}{s} \times T^{\sigma} + \frac{z}{D} \times \frac{T}{t} \times T^{\tau} + \&c.$

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T H E O R E M.

Assume (n) quantities $\alpha, \beta, \gamma, \delta, \epsilon, \&c.$ then will the sum of all the (n) quantities of the following kind

$$\begin{aligned}
 & \frac{\alpha^m}{\alpha - \beta \times \alpha - \gamma \times \alpha - \delta \times \alpha - \epsilon \times \alpha - \&c.} + \frac{\beta^m}{\beta - \alpha \times \beta - \gamma \times \beta - \delta \times \beta - \epsilon \times \beta - \&c.} \\
 & + \frac{\gamma^m}{\gamma - \alpha \times \gamma - \beta \times \gamma - \delta \times \gamma - \epsilon \times \gamma - \&c.} + \frac{\delta^m}{\delta - \alpha \times \delta - \beta \times \delta - \gamma \times \delta - \epsilon \times \delta - \&c.} \\
 & + \frac{\epsilon^m}{\epsilon - \alpha \times \epsilon - \beta \times \epsilon - \gamma \times \epsilon - \delta \times \epsilon - \&c.} + \&c. = 0, \text{ if } m \text{ be any whole} \\
 & \text{number less than } n - 1; \text{ but if } m = n - 1, \text{ then will the} \\
 & \text{above mentioned sum} = 1. \text{ In general, the sum of the} \\
 & n \text{ terms } \frac{\alpha^m (\beta \gamma \delta \epsilon \&c. + \beta \gamma \epsilon \&c. + \beta \delta \epsilon \&c. + \gamma \delta \epsilon \&c. + \&c.)}{\alpha - \beta \times \alpha - \gamma \times \alpha - \delta \times \alpha - \epsilon \times \alpha - \&c.} + \\
 & \frac{\beta^m (\alpha \gamma \delta \epsilon \&c. + \alpha \gamma \epsilon \&c. + \alpha \delta \epsilon \&c. + \gamma \delta \epsilon \&c. + \&c.)}{\beta - \alpha \times \beta - \gamma \times \beta - \delta \times \beta - \epsilon \times \beta - \&c.} + \\
 & \frac{\gamma^m (\alpha \beta \delta \epsilon \&c. + \alpha \beta \epsilon \&c. + \alpha \delta \epsilon \&c. + \beta \delta \epsilon \&c. + \&c.)}{\gamma - \alpha \times \gamma - \beta \times \gamma - \delta \times \gamma - \epsilon \times \gamma - \&c.} + \\
 & \frac{\delta^m (\alpha \beta \gamma \epsilon \&c. + \alpha \beta \epsilon \&c. + \alpha \gamma \epsilon \&c. + \&c.)}{\delta - \alpha \times \delta - \beta \times \delta - \gamma \times \delta - \epsilon \times \delta - \&c.} + \\
 & \frac{\epsilon^m (\alpha \beta \gamma \delta \&c. + \alpha \beta \delta \&c. + \alpha \gamma \delta \&c. + \beta \gamma \delta \&c. + \&c.)}{\epsilon - \alpha \times \epsilon - \beta \times \epsilon - \gamma \times \epsilon - \delta \times \epsilon - \&c.} + \&c. = 0,
 \end{aligned}$$

if m be less than n , and $m+r$ not equal to $n-1$, where r is equal to the number of letters contained in each of the contents above mentioned $\beta \gamma \delta, \&c. \beta \gamma \epsilon, \&c. \beta \delta \epsilon, \&c. \gamma \delta \epsilon, \&c. \&c. \&c.$ respectively: but if $m+r = n-1$, then will the above mentioned sum $= \pm 1$; it will be $+1$ if r be an even number, otherwise -1 .

DEMONSTRATION.

Suppose $a+b\alpha+c\alpha^2+d\alpha^3+e\alpha^4+\&c.=s^a$,

$a+b\beta+c\beta^2+d\beta^3+e\beta^4+\&c.=s^b$,

$a+b\gamma+c\gamma^2+d\gamma^3+e\gamma^4+\&c.=s^c$,

$a+b\delta+c\delta^2+d\delta^3+e\delta^4+\&c.=s^d$,

$a+b\varepsilon+c\varepsilon^2+d\varepsilon^3+e\varepsilon^4+\&c.=s^e$, multiply

these equations into A, B, C, D, E, &c. unknown co-efficients to be investigated, and there result

$$A \times s^a = Aa + Ab\alpha + Ac\alpha^2 + Ad\alpha^3 + Ae\alpha^4 + \&c.$$

$$B \times s^b = Ba + Bb\beta + Bc\beta^2 + Bd\beta^3 + Be\beta^4 + \&c.$$

$$C \times s^c = Ca + Cb\gamma + Cc\gamma^2 + Cd\gamma^3 + Ce\gamma^4 + \&c.$$

$$D \times s^d = Da + Db\delta + Dc\delta^2 + Dd\delta^3 + De\delta^4 + \&c.$$

$$E \times s^e = Ea + Eb\varepsilon + Ec\varepsilon^2 + Ed\varepsilon^3 + Ee\varepsilon^4 + \&c. \&c. \&c.$$

Now suppose $As^a + Bs^b + Cs^c + Ds^d + Es^e + \&c. = a + bx + cx^2 + dx^3 + ex^4 + \&c.$ and the correspondent parts respectively equal to each other; that is, $a(A+B+C+D+E+\&c.) = a$; $b(A\alpha+B\beta+C\gamma+D\delta+E\varepsilon+\&c.) = bx$; $A\alpha^2+B\beta^2+C\gamma^2+D\delta^2+E\varepsilon^2+\&c. = x^2$; $A\alpha^3+B\beta^3+C\gamma^3+D\delta^3+E\varepsilon^3+\&c. = x^3$; $A\alpha^4+B\beta^4+C\gamma^4+D\delta^4+E\varepsilon^4+\&c. = x^4$, &c.: But it follows from Theorem I. that (if $As^a + Bs^b + Cs^c + Ds^d + Es^e + \&c. = a + bx + cx^2 + dx^3 + ex^4 + \&c.$) $A = \frac{x-\beta \times x-\gamma \times x-\delta \times x-\varepsilon \times \&c.}{a-\beta \times x-\gamma \times x-\delta \times x-\varepsilon \times \&c.}$

$$B = \frac{x-a \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\beta-a \times \beta-\gamma \times \beta-\delta \times \beta-1 \times \&c.}, \quad C = \frac{x-a \times x-\beta \times x-\delta \times x-1 \times \&c.}{\gamma-a \times \gamma-\beta \times \gamma-\delta \times \gamma-1 \times \&c.},$$

$$D = \frac{x-a \times x-\beta \times x-\gamma \times x-1 \times \&c.}{\delta-a \times \delta-\beta \times \delta-\gamma \times \delta-1 \times \&c.}, \quad E = \frac{x-a \times x-\beta \times x-\gamma \times x-\delta \times \&c.}{1-a \times 1-\beta \times 1-\gamma \times 1-\delta \times \&c.},$$

&c.: substitute these values for A, B, C, D, E, &c. respectively in the preceding equations ($A+B+C+D+E+\&c.=1$, $A\alpha+B\beta+C\gamma+D\delta+E\epsilon+\&c.=x$, $A\alpha^2+B\beta^2+C\gamma^2+D\delta^2+E\epsilon^2+\&c.=x^2$, $A\alpha^3+B\beta^3+C\gamma^3+D\delta^3+E\epsilon^3+\&c.=x^3$, &c.)

and there result the equations (1) $\frac{x-\beta \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\alpha-\beta \times \alpha-\gamma \times \alpha-\delta \times \alpha-1 \times \&c.}$

$$+ \frac{x-a \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\beta-a \times \beta-\gamma \times \beta-\delta \times \beta-1 \times \&c.} + \frac{x-a \times x-\beta \times x-\delta \times x-1 \times \&c.}{\gamma-a \times \gamma-\beta \times \gamma-\delta \times \gamma-1 \times \&c.} + \&c.=1;$$

$$(2) \alpha \times \frac{x-\beta \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\alpha-\beta \times \alpha-\gamma \times \alpha-\delta \times \alpha-1 \times \&c.} + \beta \times \frac{x-a \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\beta-a \times \beta-\gamma \times \beta-\delta \times \beta-1 \times \&c.}$$

$$+ \gamma \times \frac{x-a \times x-\beta \times x-\delta \times x-1 \times \&c.}{\gamma-a \times \gamma-\beta \times \gamma-\delta \times \gamma-1 \times \&c.} + \&c.=x;$$

$$(3) \alpha^2 \times \frac{x-\beta \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\alpha-\beta \times \alpha-\gamma \times \alpha-\delta \times \alpha-1 \times \&c.} + \beta^2 \times \frac{x-a \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\beta-a \times \beta-\gamma \times \beta-\delta \times \beta-1 \times \&c.}$$

$$+ \gamma^2 \times \frac{x-a \times x-\beta \times x-\delta \times x-1 \times \&c.}{\gamma-a \times \gamma-\beta \times \gamma-\delta \times \gamma-1 \times \&c.} = x^2; \text{ and in general,}$$

$$\alpha^m \times \frac{x-\beta \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\alpha-\beta \times \alpha-\gamma \times \alpha-\delta \times \alpha-1 \times \&c.} + \beta^m \times \frac{x-a \times x-\gamma \times x-\delta \times x-1 \times \&c.}{\beta-a \times \beta-\gamma \times \beta-\delta \times \beta-1 \times \&c.}$$

$$+ \gamma^m \times \frac{x-a \times x-\beta \times x-\delta \times x-1 \times \&c.}{\gamma-a \times \gamma-\beta \times \gamma-\delta \times \gamma-1 \times \&c.} + \delta^m \times \frac{x-a \times x-\beta \times x-\gamma \times x-1 \times \&c.}{\delta-a \times \delta-\beta \times \delta-\gamma \times \delta-1 \times \&c.}$$

+ &c.= x^m , whatever may be the values of the quantities x ; α , β , γ , δ , ϵ , &c.: reduce all these fractions into terms, proceeding according to the dimensions of the quantity x , and it is evident, that the sum of all the fractions multiplied

multiplied into any dimension of x not equal to m will be $= 0$; but the sum of all the fractions multiplied into x^m will be $= 1$: from this proposition the theorem is easily deduced.

I have invented and demonstrated from different principles to the preceding the first part of this theorem, a particular case of which was published by me many years ago.

From this theorem may easily be deduced several others of a similar nature.

VIII. *Disquisitio de tempore Periodico Cometæ anno 1770 observati. Autore J. A. Lexell, Academiæ Scientiarum Petrop. Socio. Communicated by Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read Feb. 21,
1778.

TEMPORIBUS periodicis cometarum investigandis, quum astronomi nondum suas impenderint curas, quod hæc astra intervallo temporis, quo incolis terræ conspicua esse solent, exiguas valde portiones suarum orbitarum conficiant, ita ut exinde vix quicquam certi, de excentricitatibus istarum orbitarum concludi queat; inexpectatum omnino astronomis contigit, quod tempus periodicum cometæ anno 1770 conspicui ex observationibus concludere posse mihi visus sim, mirum autem et vix ulla fide dignum videri debuit, quod hoc tempus periodicum adeo exiguum a me inventum est, ut vix quinque annos cum dimidio superaret, ita ut hic cometa suas periodos circa solem minori adhuc tempore, quam Jupiter et Saturnus absolvere deberet. Propositum quidem mihi nunc non est, omnium calculorum, quibus ad hanc conclusionem perductus sum, adumbrationem hic tradere, quippe quod prolixum valde et pro instituto

tuto minus necessarium foret; sufficit ut argumentum proponam pro ista hypothefi temporis periodici stabilienda, meo quidem judicio ita stringens, ut demonstrationi geometricæ æquiparari queat. Hujus autem argumenti vis, in eo consistit, ut elementis adhibitis, quæ tempori periodico quinque annorum et septem mensium, conformia sunt, observationibus hujus cometæ optime satisfieri demonstretur, contra vero si tempus periodicum cometæ majus supponatur, insignes et vix quidem probabiles errores observationibus induci. Sunt igitur elementa, pro motu cometæ a me stabilita, sequentia:

1. Longitudo nodi ascendentis $4^{\circ} 12' 0''$.

2. Inclinatio orbitæ ad eclipticam $1^{\circ} 33' 40''$.

3. Elongatio nodi descendentis a perihelio $44^{\circ} 17' 4''$, ideoque longitudo perihelii $11^{\circ} 26' 16''$.

4. Tempus transitus per perihelium anno 1770 die 13 Aug. $13^h 5'$ circiter, five 13,5450 Aug.

5. Cometæ distantia perihelii 0,6743815, cujus log. = 9,8289057.

6. Semiaxis orbitæ a cometa descriptæ = 3,1478606, cujus log. = 0,4980155. Hinc log. semiparametri = 0,0807300, et log. excentric. = 9,8952927, ideoque tempus periodicum 5,585 annorum.

His elementis adhibitis, sequentes comparationes locorum cometæ ex theoria deductorum, cum observatis, habebuntur.

Jun.

	Temp. med. Paris.	Long. com. obfer.	Long. com. ex calculo.	Diff.	Latit. com. obferat.	Latit. com. ex calculo.	Diff.
Jun. 14	11 29 48	9 2 47 44	9 2 48 1	+0 17	6 40 24B	6 40 54B	+0 30
15	11 23 22	9 2 51 49	9 2 51 54	+0 5	6 57 15	6 57 51	+0 36
17	11 11 33	9 2 59 58	9 3 0 52	+0 54	7 38 51	7 38 37	-0 14
20	10 40 48	9 3 16 12	9 3 17 2	+0 50	9 5 38	9 5 29	-0 9
21	10 27 45	9 3 23 17	9 3 24 26	+1 9	9 44 36	9 45 27	+0 51
22	12 9 36	9 3 33 43	9 3 34 9	+0 26	10 39 5	10 39 54	+0 49
24	12 3 18	9 3 59 58	9 3 59 51	-0 7	12 59 55	12 59 52	-0 3
25	13 27 55	9 4 21 42	9 4 21 21	-0 21	14 53 56	14 54 37	+0 41
27	13 13 17	9 5 34 54	9 5 36 6	+1 12	21 8 47	21 8 38	-0 9
28	10 46 34	9 6 45 58	9 6 43 58	-2 0	26 29 33	26 30 39	+1 6
29	11 59 26	9 9 42 45	9 9 42 30	-0 15	37 57 32	38 0 37	+3 5
Aug. 2	15 3 15	3 6 2 32	3 6 2 4	-0 28	0 50 7A	0 49 59A	-0 8
3	14 45 9	3 6 25 15	3 6 24 32	-0 43	0 53 36	0 52 46	-0 50
4	14 12 48	3 6 47 24	3 6 47 46	+0 18	0 55 44	0 56 4	+0 20
5	14 38 43	3 7 13 53	3 7 13 3	-0 48	0 59 17	0 58 50	-0 27
6	14 29 42	3 7 39 25	3 7 38 45	-0 40	1 1 18	1 1 16	-0 2
7	14 49 19	3 8 6 40	3 8 5 59	-0 41	1 3 35	1 3 34	-0 1
8	14 20 13	3 8 33 29	3 8 33 17	-0 12	1 6 36	1 5 35	-1 1
9	14 48 16	3 9 4 30	3 9 2 40	-1 50	1 9 33	1 7 29	-2 4
10	14 14 27	3 9 32 36	3 9 31 44	-0 52	1 9 13	1 9 9	-0 4
11	14 23 23	3 10 3 23	3 10 2 37	-0 25	1 10 51	1 10 40	-0 11
12	14 46 25	3 10 34 48	3 10 34 43	-0 5	1 12 52	1 12 4	-0 48
14	14 37 29	3 11 40 25	3 11 40 4	-0 21	1 15 25	1 14 25	-1 0
15	15 43 33	3 12 14 56	3 12 15 37	+0 41	1 16 43	1 15 26	-1 17
18	14 24 32	3 13 59 24	3 13 59 37	+0 13	1 18 35	1 17 39	-0 56
19	14 33 18	3 14 36 33	3 14 36 16	+0 13	1 19 18	1 18 37	-0 41
26	15 39 38	3 19 3 44	3 19 3 59	+0 15	1 20 45	1 20 5	-0 40

Aug

	Temp. med. Paris.	Long. com. observ.	Long. ex calculo.	Diff.	Latit. com. observat.	Latit. ex calculo.	Diff.
	h / "	o / "	o / "	/' "	o / "	o / "	/' "
Aug. 28	14 44 8	3 20 19 42	3 20 20 27	+0 45	1 20 55A	1 20 5A	-0 50
29	15 21 53	3 21 0 3	3 21 0 31	+0 28	1 20 5	1 20 3	-0 2
30	14 48 22	3 21 39 12	3 21 38 44	-0 28	1 20 56	1 19 58	-0 58
31	14 38 25	3 22 17 16	3 22 17 49	+0 33	1 20 23	1 19 50	-0 33
Sept. 4	15 5 20	3 24 52 41	3 24 53 46	+1 5	1 19 23	1 19 2	-0 21
5	14 48 37	3 25 31 7	3 25 31 44	+0 37	1 19 45	1 18 47	-0 58
8	15 57 43	3 27 26 32	3 27 27 41	+1 9	1 18 23	1 17 51	-0 32
9	15 6 30	3 28 3 19	3 28 3 52	+0 33	1 18 22	1 17 33	-0 49
10	16 26 33	3 28 40 26	3 28 43 15	+2 49	1 19 4	1 17 12	-1 52
14	14 16 26	4 1 4 36	4 1 5 35	+0 59	1 16 18	1 15 46	-0 32
17	15 53 34	4 2 52 40	4 2 53 13	+0 33	1 14 52	1 14 35	-0 17
18	15 30 16	4 3 26 57	4 3 27 6	+0 9	1 14 58	1 14 12	-0 46
19	15 19 44	4 4 0 32	4 4 0 37	+0 5	1 13 2	1 13 48	+0 46
20	15 33 45	4 4 34 17	4 4 34 27	+0 10	1 12 35	1 13 35	+1 0
29	15 23 51	4 9 14 54	4 9 15 53	+0 59	1 10 16	1 9 42	-0 34
Oct. 1	15 23 22	4 10 12 6	4 10 13 31	+1 25	1 9 51	1 8 54	-0 57
—	16 33 28	4 10 13 54	4 10 14 55	+1 1	1 10 4	1 8 53	-1 11
2	15 43 37	4 10 40 51	4 10 41 59	+1 9	1 10 4	1 8 29	-1 35
—	16 38 50	4 10 41 52	4 10 43 51	+1 13	1 10 10	1 8 29	-1 41

Consensus itaque theoriæ cum observationibus jam quidem tantus est, ut vix majorem desiderare liceat, nam pro longitudinibus aberratio nunquam duo minuta prima supergreditur, nisi pro observatione die 10 Septembris instituta, quæ tamen observatio, uti ex comparatione cum reliquis patefiet, non potest non aliquantum esse dubia.

Pro

Pro latitudinibus quoque, errores etiam si plerumque negativum fortiantur valorem, majores tamen non sunt, quam ut verisimiles videri debeant; nam differentia pro observatione diei 29 Junii, quæ est 3 minutorum, ope parallaxeos cometæ, quæ tum temporis insignis erat, facilem admittit explicationem. Cæterum facile tamen intelligitur, elementa observationibus satisficientia cum aliqua latitudine assumi posse. Sic si tempus periodicum statuatur 5, 6 annorum, log. semiparam. orbitæ = 0,0808000, log. dist. perih. 9,8288794, tempus perihelii = 13,5400 Aug. long. $\alpha = 4^s 12^o 9'$, inclinatio orbitæ $1^o 33' 40''$, elongatio a $\alpha = 44^o 7' 59''$, erunt loca cometæ ex calculo.

	.	•	'	"		•	'	"
Pro 15 Jun. long.	9	2	52	12	lat.	6	58	37 Bor.
29	9	9	42	59		38	0	54
2 Aug.	3	6	2	7		38	49	35 A.
29	3	21	0	33		1	20	1
1 Octob.	4	10	13	20		1	9	0

Quæ loca æque bene, vel aliquanto melius cum observatione consentiunt, ac quæ supra attulimus. Nunc igitur dispiciamus an tempus periodicum insignius auctum, elementis inveniendis inserviat quæ cum observationibus componi queant. Supponamus igitur primum omnes observationes ad unam eandemque orbitam cometæ pertinere,

tinere, seu quod idem est, cometam in approximatione sua ad tellurem, ab actione telluris affectum non fuisse; deinde enim visuri erimus, quid statuendum sit, si supponatur actionem telluris in motum cometæ aliquem habuisse influxum. Jam igitur assumpto certo tempore periodico, uti primum 6 annorum, et adhibito certo valore pro semiparametro orbitæ, in reliqua ejus elementa inquisivi quæ ita essent comparata, ut observationibus diebus 15 et 29 Junii institutis, satisfacerent; patet enim quod si bina cometæ elementa, uti tempus periodicum et parameter orbitæ, pro cognitis habeantur, omnia quæ ad motum cometæ stabiliendum desiderantur, ope duarum observationum investigari posse.

Positis itaque nunc, tempore periodico 6 annorum, et $\log. \text{semipar.} = 0,0817000$, reliqua elementa erunt; $\log. \text{dist. perih.} = 9.8273218$, $\text{temp. perih.} = 13,2850$ Augusti, $\text{long. } \alpha = 4^{\circ} 12' 6''$, $\text{inclinatio orbitæ} = 1^{\circ} 34' 30''$, $\text{elong. perih. a } \vartheta = 44^{\circ} 9' 56''$. Hincque loca cometæ ex calculo.

	.	°	'	"	.	'	"
Pro 15 Jun. long.	9	2	51	52	lat. 6	58	6 Bor.
29	9	9	43	6	38	0	27
2 Aug.	3	6	3	18	38	50	15
29	3	21	5	42	1	20	6
1 Oct.	4	10	11	54	1	9	10

Supposito iterum tempore periodico 6 annorum et log. femipar. = 0,0818500 erit tempus perihelii 13,2900 Aug. long. $\alpha = 4^s 12^\circ 28'$, inclinatio orbitæ = $1^\circ 34' 2''$, elong. perihel. a $\varpi = 43^\circ 37' 28''$, tumque erunt:

	Comet. longit.				Latitud.		
	°	'	''		°	'	''
Pro 15 Jun.	9	2	51	25	6	58	6 Bor.
29	9	9	42	42	38	0	24
2 Aug.	3	6	2	47	0	49	3 Auf.
29	3	21	4	56	1	19	36
1 Oct.	4	10	10	59	1	9	1

Pro hac igitur posteriori hypothefi, observationibus dierum 2 et 29 Aug. respectu longitudinis quidem melius fatisfit, quam per priorem hypothefin, ac observatio die 1 Octobr. respectu longitudinis jam magis evadit erronea, tumque etiam respectu latitudinis errores aliquantum augentur; concludi autem hinc quoque potest, quæcunque etiam suppositio fiat pro femiparametro orbitæ si tempus periodicum statuatur 6 annorum, et observationibus diebus 15 et 29 Junii satisfaciendum fit, observationes occurrere, quæ erroribus saltem duorum minutorum, tam negativis quam positivis afficientur. Quod si jam supponatur tempus periodicum 7 annorum et log. femiparam. = 0,0837000, erit tempus perihelii 12,7950 August. long. $\alpha = 4^s 12^\circ 49'$, inclinatio orbitæ

2

= 1°

$= 1^{\circ} 35' 30''$, elongatio perihelii a $\alpha = 43^{\circ} 26' 10''$, tumque habebuntur:

	Com. longit.	Latitudo.
	° ' "	° ' "
Pro 15 Jun.	9 2 52 1	6 58 16 Bor.
29	9 9 42 50	38 0 28
2 Aug.	3 6 6 19	38 49 3 Aufst.
29	5 21 16 18	1 19 31
1 Oct.	4 10 9 22	1 9 17

Deinde adhibito iterum tempore periodico 7 annorum et log. femipar. $= 0,0840000$, erit tempus perihelii $= 12,8050$ Aug. long. $\alpha = 4^{\circ} 13' 58''$, inclinatio orbitæ $= 1^{\circ} 33' 50''$, et long. perih. a $\gamma = 42^{\circ} 14' 41''$, hincque sequuntur.

	° ' "	lat. ° ' "
Pro 15 Jun. long.	9 2 51 54	6 58 30 B.
29	9 9 42 38	38 0 30
2 Aug.	3 6 5 59	38 45 25 A.
29	3 21 13 54	1 17 57
1 Oct.	4 10 6 2	1 9 13

Posteriori adhibita hypothesi, errores observationum die 2 et 29 Aug. institutarum, quoad longitudinem aliquantum minuuntur, in latitudinem autem tanto majores, redundant, tumque observatio die 1 Oct. instituta secundum posteriorem hypothesin multo magis redditur erronea. Certum igitur est cum tempore periodico septem

annorum omnes observationes circa cometam anni 1770 nulla ratione impleri posse; quin potius adeo enormes errores in nonnullas earum redundare, ut vix ullam invenire queant fidem. Restat autem nunc, ut examinemus, an ne saltem omnibus observationibus a die 2 Augusti usque ad 2 Octobris institutis, satisfieri queat; adhibito tempore periodico aliquantum majori. Et tum quidem evidens est, quia arcus cometæ circa solem descriptus a 2 Augusti usque ad 2 Octobris, multo minor est illo, quem a 15 Jun. ad 2 Octobris percurrit, tempus periodicum quo observationibus secundæ apparitionis satisfaciendum sit, jam cum majori latitudine assumi posse, quam si omnibus in universum observationibus satisfaciendum esset. Hinc igitur si tempus periodicum statuetur 6 annorum, ejusmodi quidem elementa facile inveniri possunt, ut observationes a 2 Aug. ad 2 Octobris factæ, saltem absque gravioribus erroribus, impleantur. Dispiciamus itaque quid evenire debeat, si tempus periodicum statuatur 7 annorum. Generatim igitur quum compertum mihi fuerit, latitudinibus cometæ facile satisfieri, modo longitudinibus fuerit satisfactum, operam dedi, ut tribus cometæ longitudinibus observatis satisfacerem; tumque examinaui qualis error in quartam aliquam observationem redundaret. Quatuor autem observationes, quarum ubique usum feci sunt illa, quæ diebus 2, 12, 29 Aug. et 1 Oct. institutæ.

institutæ habentur. Primum igitur elementa investigavi, quibus observationes dierum 2, 29 August. et 1 Octobris implentur, quæ sequentia mihi se obtulerunt.

Log. femipar. = 0,0925000, tempus perih. = 15,6280 August, elong. perih. a $\vartheta = 46^{\circ} 14' 6''$, posita longitudine $\alpha = 4^{\circ} 12' 0''$, loca autem cometæ ex theoria deducta, nunc ita se habebunt :

Longitudo cometæ.

Pro	2 Aug.	3	6	2	27.
	12	3	10	41	45
	29	3	21	0	20.
	1 Oct.	4	10	12	29.

Ubi in observatione die 12 Augusti facta reperitur error 7 minutorum primorum. Deinde aliis calculis comperi, quod si sub hypothese temporis periodici 7 annorum observationes dierum 2 Aug. et 1 Octobris impleantur, quicumque demum error in observatione die 29 Augusti admittatur, errorem observationis die 12 Augusti factæ, nunquam infra 7 minuta prima deprimi posse; unde tanto magis concludere licet, aucto tempore periodico, observationes dierum 2, 12 Aug. et 1 Octob. multo minus inter se componi posse. Ulterius pergendo, operam dedi ut observationes diebus 12 et 29 Aug. atque 1 Oct. factas implerem, quod sequentibus elementis obtinui.

Log.

Log. femi-par. = 0,0915000, temp. perih. = 15;1340.
 Aug. elong. perih. a ϑ = $45^{\circ} 59' 24''$, posita longitudine
 nodi ut supra, tum enim erant:

		Longitud. cometæ.			
		.	°	'	"
Pro	2 Aug.	3	5	49	1
	12	3	10	34	41
	29	3	21	0	20
	1 Oct.	4	10	12	6

Ubi observatio die 2 Augusti facta, 13 minutis primis redditur erronea, qui etiam minimus fere error est, quem hæc observatio admittit, dum observationes dierum 12 Aug. et 1 Octob. sub hypothesi temporis periodici septem annorum implendæ sunt. Deinde si idem adhibeatur tempus periodicum, et observationibus dierum 2, 12, et 29 Aug. satisfactum sit, quod fiet ponenda log. semipar. = 0,0837600, tempus perih. = 12,1500 August, elongat. a ϑ = $43^{\circ} 16' 33''$, longit. nodi ut supra, in observatione die 1 Octobris instituta, reperietur error 35 min. prim. Omnes autem hæc disquisitiones eo tendunt, ut perficiatur, observationes a 2 Aug. usque ad 2 Octob. institutas, sub hypothesi temporis periodici 7 annorum nunquam perfecte impleri posse, sed inter illas saltem nonnullas occurrere, quæ erroribus 7 minut. primorum obnoxixæ sunt, quod quidem vix ulla verisimilitudine

tudine gaudet. Quamvis hinc facile concludi possit, aucta quantitate temporis periodici, errores observationum augeri, ipsemet tamen calculo instituto ea de re certior fieri volui. Posito itaque tempore periodico 8 annor. log. semipar. = 0,1000000, tempore perih. = 17,2300, elong. perih. a ϑ = $47^{\circ} 32' 4''$, long. α ut supra adhibita sequentes inveni longitudes cometæ.

		.	°	'	"
Pro 2 Aug.	3	6	2	44	
12	3	10	47	42	
15	3	12	26	33	
29	3	21	0	57	
1 Oct.	4	10	12	6	

Ubi jam in observatione die 12 Aug. instituto, occurrit error 13 minutorum primorum. Tum vero me non monente intelligitur, quod supposita tempore periodico septem annorum, quæcunque demum elementa adhibeantur inter illa, quæ observationibus secundæ apparitionis satisfacere debent, inde enormes omnino errores pro observationibus primæ apparitionis emergere. Operæ quidem pretium non esse judicavi, ut inquirerem, an valor temporis periodici allatus sensibilem admittat diminutionem, quia hoc unicuique minus probabile videbitur; ex iis autem, quæ jam attuli, facile colligitur, si omnibus in universon observationibus satisfaciendum sit, tempus periodicum

periodicum allatum vix dimidia parte anni infra illum valorem, quem supposui, deprimi posse; sin autem observationibus secundæ apparitionis solummodo satisfaciendum sit, omnes quidem valores temporis periodici intra limites 5 et 6 annorum contenti, absque metu sensibilis erroris in observationes redundantis admitti possent. Existimaverim tamen hos limites nullo modo ultra quatuor annos cum dimidio et 6 annos cum dimidio prorogari posse, quin potius maxime mihi est vero simile, valorem temporis periodici, arctioribus istis limitibus 5, vel 6 annorum circumscribi.

Quo autem certius est argumentum; quod pro stabiliendo tempore periodico cometæ anno 1770 observati, jam proposui, eo sane magis unicuique mirum videri debet, quod hunc eundem cometam non nisi unica vice observare licuerit. Nam si hic cometa singulis quinque annis et septem mensibus ad suum rediret perihelium, utique fieri debuisse videtur, ut saltem hoc seculo, postquam studium cœlorum majori assiduitate cœli coeptum est, sæpius se conspiciendum præbuisset. Cum variæ quidem conjecturæ proponi possent pro explicando eventu adeo singulari, tum inter illas eam adferre, quæ maxima probabilitate se commendat, hac occasione sufficiet. Quum distantia aphelia cometæ a sole, distantiam Jovis ab hoc astro fere exæquet, statim suspicio quædam suboritur,

fuboritur, an non fieri potuit, ut per actionem Jovis, motus cometæ olim fuerit perturbatus, ita ut cometa hic antehac orbitam, a præfenti multo diverfam, descripferit. Calculo autem instituto reperitur, cometam fuiſſe in conjunctione cum Jove, Anno 1767, die 27 Maii, eorumque diſtantiã tunc temporis fuiſſe 58vã partem diſtantiæ cometæ a ſole, unde habito reſpectu maſſarum Solis et Jovis, colligitur actionem Jovis in cometam fuiſſe triplo majorem actione Solis, ideoque ſenſibilem omnino effectum ad motum cometæ perturbandum producere valuiſſe; eo potius quod in aphelio cometa motu admodum lento feratur, adeoque ſatis diu actioni Jovis fuerit expoſitus. Ulterius per elementa ſupra a nobis ſtabilita colligitur, proxime futuram conjunctionem Jovis cum cometa contingere debere anno proxime ſequenti die 23 Auguſti, exiſtente tunc diſtantiã cometæ a Jovẽ non niſi 491^{ma} parte diſtantiæ cometæ a ſole, ideoque actionem Jovis in cometam actione Solis 224 vicibus majorem eſſe, unde totalis mutatio motus cometæ non poterit non oriri. Cæterum hæ concluſiones pro exacte veris haberi non poterunt, niſi quatenus elementa pro motu cometæ allata, fuerint exactiſſime ſtabilita; quippe quum leviffima mutatio horum elementorum, imprimifque temporis periodici, diſtantiã cometæ apheliam ſenſibili mutatione afficiat, et per facilem calculum exploravi

quod si tempus periodicum cometæ aliquantulum diminuat, eo effici ut actio Jovis in priori conjunctione augeatur, contra vero in posteriori conjunctione diminuat. Hoc igitur ratiocinio id præ-primis mihi fuit propositum, ut ostenderem, fieri utique potuisse, quod hæc cometa ob actionem Jovis coactus fuerit ejusmodi orbitam describere, quæ ex observationibus anno 1770 factis colligitur, licet antea motum suum perfecit in orbita, cujus tempus periodicum satis insigne esse potuerit. Utrum cometa noster in approximatione sua ad tellurem, ab actione telluris fuerit affectus, id quidem nec affirmare nec negare ausim, saltem verisimile mihi videtur, hanc actionem non admodum sensibiles mutationes producere valuisse, et ex superioribus quidem constat, pro tempore periodico cometæ non admodum magnas alterationes oriri potuisse.

Denique licet valde dubium esse queat, utrum cometa noster in proximo ad perihelium accessu, nobis se conspiciendum præbeat, quod motus ejus ab actione Jovis totalem forsan subierit mutationem; tamen astronomis haud prorsus ingratum esse existimaverim, si tabulam subjunxerim, quæ ostendet in quibusnam cœli regionibus singulis mensibus cometam hunc quærere oporteat, respectu quoque habito ad majores vel minores valores temporis periodici, limitibus 5 et 6 annorum contentos.

	Long.	Latit.	Long.	Latit.	Long.	Latit.	Long.	Latit.	Long.	Latit.
Maii	0 13	0 14A	11 12	0 49B	10 5	2 7B	8 27	2 55B		
15	0 5	0 20B	10 26	2 6	9 1	3 29	7 29	3 13		
Jun.	1 3	0 18A	11 27	1 33	9 3	4 41	7 22	3 25		
15	1 0	0 30B	9 4	7 7	7 12	3 18	7 3*	2 38		
Jul.	1 26	0 25A	in conj. cum ☉		7 5	2 56	7 0*	2 17		
15	in conj. cum ☉		6 29	2 29	6 29*	1 59				
Aug.	3 5	0 49A	6 24	1 37	7 1*	1 36				
15	3 12	1 16	6 23	0 51	7 4*	1 17				
Sept.	3 23	1 19	6 1	0 58A	7 10	0 50				
15	4 1	1 15	5 8	1 10	7 16	0 27				
Oct.	4 10	1 9	4 22	2 23	7 24	0 31				
15	4 21	2 13	7 27	1 34						
Nov.	4 22	1 58	7 14	5 8	8 22	1 5				
15	4 20	1 52	6 25	6 1	9 6	0 54				
Dec.	4 10	1 37	4 15	7 44	10 4	5 42	10 20	2 55A		
15	4 0	1 20	3 22	4 31	9 3	19 7	10 29	3 14	9 28	0 39A
Jan.	3 5	2 36	0 13	3 12	10 28	1 16A				
15	3 0	1 42	1 10	2 19	11 23	1 32	10 18*	0 31		
Feb.	3 1	1 2	2 5	1 0	0 23	1 26	11 19*	0 57		
15	3 4*	0 40	0 10*	1 2	11 8*	0 28	10 14*	0 9B		
Mar.	0 3*	0 44	11 6*	0 12	10 12*	0 28B				
15	0 24	0 49	11 24*	0 29	11 1*	0 19				
Apr.	11 25*	0 12	10 28*	0 33B	10 1*	1 21				
15	0 16*	0 29	11 18*	0 12	10 17*	1 13				

De hac tabula notari convenit, quod in illa saltus non
nunquam majores prodeant, quod inde evenit, quia ex una
columna verticali in præcedentem transitus factus fit;

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fic si supponatur tempus perihelii incidere in 13 Junii, erit pro 1 Junii longitudo cometæ $11^{\circ} 27'$ et latitudo $1^{\circ} 33'$, quæ in columna verticali secunda allegantur; pro 15 autem Junii, erit longitudo $1^{\circ} 0'$ et latitudo $30'$ bor. quæ in columna verticali prima afferuntur. Fundamentum autem cui constructio hujus tabulæ innititur, in eo positum est, quod supposuerim cometam si incolis telluris conspicuus evadat, non multo magis a terra debere esse remotum, ac erat dum anno 1770 mense Octobris conspicuus esse desiit, hincque etiam loca in tabula nostra asteriscis notata indicant, dubium esse, an cometa in his locis visibilis fiat.

IN a pamphlet of eighteen pages in quarto, published at Upsal in 1776, Mr. ERIC PROSPERIN, member of the Royal Academies of Sciences at Stockholm and Upsal, and Astronomer to the king of Sweden, has shewn by his calculations, that the observations of near four months made on this comet by M. MESSIER could not be represented by a parabolic orbit; and founds a strong conjecture thereon, and on the circumstances of the different parabolas which he found necessary to represent the motion of the comet at different periods of time during its appearance,

appearance, that its orbit may be sensibly elliptical (which it seems M. PINGRE, who first calculated the orbit in a parabola, had also some suspicion of) and concludes with recommending the investigation of the true elements of its orbit in an ellipsis. The laborious calculation thus recommended has, we see, been since successfully and satisfactorily performed in this paper by Mr LEXELL.

N. MASKELYNE.

IX. *On the General Resolution of Algebraical Equations.*
 by Edward Waring, M. D. F. R. S. and of the Institute
 of Bononia, *Lucasian Professor of Mathematics in the*
University of Cambridge.

Read Jan. 28, 1779.

IN the year 1757 I sent some papers to the Royal Society, which papers were printed in the year 1759, and copies of them delivered to several persons; these papers somewhat corrected, with the addition of a second part on the properties of curve lines, were published in the year 1762. In the years 1767, 1768 and 1769 I printed, and published in the beginning of the year 1770, the same papers with additions and emendations under the title of *Meditationes Algebraicæ*. In these papers were contained, with many other inventions, the most general resolution of algebraical equations known, as it contains the resolution of every algebraical equation, of which the general resolution has been given, viz. the resolution of quadratic, cubic and biquadratic, the resolution of Mr. DE MOIVRE's and Mr. BEROUT's (since published) equations; it discovers the
 resolution

resolution of an equation of n dimensions, of which the n roots are given, and also deduces innumerable equations of n dimensions, which contain $n - 1$ independent coefficients. From whence it seems probable, that this new method of mine may contain the most general resolution of algebraical equations that ever has, or, perhaps, ever will be invented.

The general resolution is $x = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + d\sqrt[n]{p^4} + \dots + r\sqrt[n]{p^{n-3}} + s\sqrt[n]{p^{n-2}} + t\sqrt[n]{p^{n-1}} + \frac{A}{x}$, if the equation be $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - \&c. = 0$.

I shall add the resolution of some particular equations from this method, and then subjoin the equation to which $x = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \&c.$ is the general resolution.

1. Let the resolution be $x = a\sqrt[3]{p} + b\sqrt[3]{p^2}$, and the correspondent equation free from radicals will be found $x^3 - 3abpx - a^3p - b^3p^2 = 0$. Let $x^3 - Px - Q = 0$ be a cubic equation whose resolution is required, which suppose the same as the equation found above, and consequently their correspondent terms equal, i. e. $P = 3abp$ and $Q = a^3p + b^3p^2$, whence $p = \frac{P}{3ab}$, which value being substituted for p in the second equation, there results $Q = \frac{Pa^2}{3b} + \frac{bP^2}{9a^2}$. In this equation for a or b may be assumed unity, or any other

other quantity whatever, and there will result an equation of the formula of a quadratic from which the other b or a may be found, whence from the equation ($p = \frac{P}{3ab}$) p may be deduced, and consequently the resolution of the cubic required.

In the same manner for p may be assumed any quantity whatever, and in the equation $Q = a^3p + b^3p^2$ for b substitute its value $\frac{P}{3ap}$, or for a its value $\frac{P}{3bp}$, and there result the equations $Q = a^3p + \frac{P^3}{27a^3p}$, and $Q = \frac{P^3}{27b^3p^2} + b^3p^2$, which have the formula of a quadratic, from which may be deduced the resolution of the cubic required.

2. Let the resolution assumed be $x = a\sqrt[4]{p} + b\sqrt[4]{p^2} + c\sqrt[4]{p^3}$; exterminate the irrational quantities, and there results the equation $x^4 - (2b^2 + 4ac)px^2 - 4(a^2bp + bc^2p^2)x - a^4p + b^4p^2 - c^4p^3 + 2a^2c^2p^2 - 4ab^2cp^2 = 0$; suppose $p = 1$, and the given equation $x^4 + qx^2 - rx + s = 0$, let the correspondent terms of the given and resulting equations be respectively made equal to each other, and there result the three equations $2b^2 + 4ac = -q$, $4b(a^2 + c^2) = r$, and $a^4 - b^4 + c^4 - 2a^2c^2 + 4ab^2c = -s$; reduce these equations into one, so that the unknown quantities a and c may be exterminated, and there results the equation $4b^6 + 2qb^4 + (\frac{q^2}{4} - s)b^2 - \frac{r^2}{16} = 0$ of the formula of a cubic, from which

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the unknown quantity b may be found, which being substituted for its value (b) in the preceding equations, from the equations thence ensuing may be found the unknown quantities a and c , and consequently the resolution of the given biquadratic $x^4 + qx^2 - rx + s = 0$.

From the same principles can be deduced different resolutions of the above-mentioned biquadratic $x^4 + qx^2 - rx + s = 0$.

3. I. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p}$, then will the equation free from radicals be $x^{2n} - 2b^n p x^n - \frac{n}{1.2} \cdot 2nb^{n-1} a^2 p x^{n-1} - \frac{n \times n^2 - 1}{1.2.3.4} \times 2nb^{n-2} a^4 p x^{n-2} - \frac{n \times n^2 - 1 \times n^2 - 4}{1.2.3.4.5.6} \times 2nb^{n-3} a^6 p x^{n-3} - \frac{n \cdot n^2 - 1 \cdot n^2 - 4 \cdot n^2 - 9}{1.2.3.4.5.6.7.8} \times 2nb^{n-4} a^8 p x^{n-4} \dots - \frac{n \cdot n^2 - 1 \times n^2 - 4 \times n^2 - 9 \cdot n^2 - 16 \dots n^2 - n - 2}{1.2.3.4.5.6.7 \dots 2n-2} \times 2na^{2n-2} b p x = a^n p - b^n p^2$.

This equation may be deduced from the following principles. Let $\alpha, \beta, \gamma, \delta, \epsilon$, &c. be the $2n$ roots of the equation $x^{2n} - 1 = 0$, then (by Prop. XXIII. of my Meditat. Algebraicæ) the equation free from radicals will be the product of the following quantities $(x - a\alpha\sqrt[n]{p} - b\alpha^2\sqrt[n]{p^2})(x - a\beta\sqrt[n]{p} - b\beta^2\sqrt[n]{p^2})(x - a\gamma\sqrt[n]{p} - b\gamma^2\sqrt[n]{p^2})(x - a\delta\sqrt[n]{p} - b\delta^2\sqrt[n]{p^2})(x - a\epsilon\sqrt[n]{p} - b\epsilon^2\sqrt[n]{p^2})$ &c. = 0 : multiply these quantities into each other, and from the resulting product, by Prob. III. of the Meditat. Algebr. easily can

be deduced the equation free from radicals which was to be found.

3. II. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^2}$, then will the correspondent equation free from radicals be $x^{2n+1} - 2n+1 \overline{b^n a p x^n} - \frac{n \cdot n+1}{1 \cdot 2 \cdot 3} \times \overline{2n+1 b^{n-1} a^3 p x^{n-1}} - \frac{n \times n^2 - 1 \times n + 2}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} \times \overline{2n+1 b^{n-2} a^5 p x^{n-2}} - \frac{n \times n^2 - 1 \times n^2 - 4 \times n + 3}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} \times \overline{2n+1 b^{n-3} a^7 p x^{n-3}} - \frac{n \cdot n^2 - 1 \cdot n^2 - 4 \cdot n^2 - 9 \cdot n + 4}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} \times \overline{2n+1 b^{n-4} a^9 p x^{n-4}} - \dots - \frac{n \cdot n^2 - 1 \cdot n^2 - 4 \cdot n^2 - 9 \cdot n^2 - 16 \dots n^2 - n - 1^2}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9 \dots 2n-1} \times \overline{2n+1 b a^{2n-1} p x} = a^{2n+1} p + b^{2n+1} p^2.$

This may be derived from the same principles as the preceding.

3. III. In general let the equation be $x = a\sqrt[m]{p} + b\sqrt[m]{p^2}$, then will the equation free from radicals become $x^m - m a^{m-2} b p x - m \cdot \frac{m-3}{2} a^{m-4} b^2 p x^2 - m \cdot \frac{m-4}{2} \cdot \frac{m-5}{3} a^{m-6} b^3 p x^3 - m \cdot \frac{m-5}{2} \cdot \frac{m-6}{3} \cdot \frac{m-7}{4} a^{m-8} b^4 p x^4 - m \cdot \frac{m-6}{2} \cdot \frac{m-7}{3} \cdot \frac{m-8}{4} \cdot \frac{m-9}{5} a^{m-10} b^5 p x^5 - \&c. = a^m p \pm b^m p^2$; if m denotes an even number, it will be $-b^m p^2$, but if an odd number, it will be $+b^m p^2$.

4. I. Let n denote an odd number, and $x = a\sqrt[n]{p} + b\sqrt[n]{p^2}$, then will $x^n - p (n a^{n-3} b x^2 + n \cdot \frac{n-5}{2} a^{n-6} b^2 x^4 + n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^{n-9} b^3 x^5 + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^{n-12} b^4 x^8 + n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{n-15} b^5 x^{10} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{n-18} b^6 x^{12} + n \dots$

$$\begin{aligned} & \frac{n-15}{2} \cdot \frac{n-16}{3} \cdot \frac{n-17}{4} \cdot \frac{n-18}{5} \cdot \frac{n-19}{6} \cdot \frac{n-20}{7} a^{\frac{n-21}{2}} b^7 x^{14} + \&c.) \pm p^2 (na^{\frac{n-1}{2}} \\ & b^{\frac{n+1}{2}} x - \frac{1}{2^2} \times n \cdot \frac{n-5}{2} \cdot \frac{n-7}{3} a^{\frac{n-9}{2}} b^{\frac{n+3}{2}} x^3 + \frac{1}{2^4} \times n \cdot \frac{n-7}{2} \cdot \frac{n-9}{3} \cdot \frac{n-11}{4} \cdot \frac{n-13}{5} \\ & a^{\frac{n-15}{2}} b^{\frac{n+5}{2}} x^5 - \frac{1}{2^6} \times n \cdot \frac{n-9}{2} \cdot \frac{n-11}{3} \cdot \frac{n-13}{4} \cdot \frac{n-15}{5} \cdot \frac{n-17}{6} \cdot \frac{n-19}{7} a^{\frac{n-21}{2}} b^{\frac{n+7}{2}} x^7 + \\ & \frac{1}{2^8} \times n \cdot \frac{n-11}{2} \cdot \frac{n-13}{3} \cdot \frac{n-15}{4} \cdot \frac{n-17}{5} \cdot \frac{n-19}{6} \cdot \frac{n-21}{7} \cdot \frac{n-23}{8} \cdot \frac{n-25}{9} a^{\frac{n-27}{2}} b^{\frac{n+9}{2}} x^9 - \\ & \&c.) = a^n p + b^n p^3. \end{aligned}$$

The quantity $\pm p^2$ denotes $+p^2$ if $\frac{n-3}{4}$ is a whole number, otherwise $-p^2$.

4. II. Let n denote an even number, and x as before $= a\sqrt[n]{p} + b\sqrt[n]{p^3}$, then will $x^n - p (na^{\frac{n-3}{2}} bx^2 + n \cdot \frac{n-5}{2} a^{\frac{n-6}{2}} b^2 x^4 + n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^{\frac{n-9}{2}} b^3 x^6 + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^{\frac{n-12}{2}} b^4 x^8 + n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{\frac{n-15}{2}} b^5 x^{10} + \&c.) \pm p^2 (\frac{1}{2} n \cdot \frac{n-4}{2} a^{\frac{n-6}{2}} b^{\frac{n+2}{2}} x^2 - \frac{1}{2^3} \times n \cdot \frac{n-6}{2} \cdot \frac{n-8}{3} \cdot \frac{n-10}{4} a^{\frac{n-12}{2}} b^{\frac{n+4}{2}} x^4 + \frac{1}{2^5} \times n \cdot \frac{n-8}{2} \cdot \frac{n-10}{3} \cdot \frac{n-12}{4} \cdot \frac{n-14}{5} \cdot \frac{n-16}{6} a^{\frac{n-18}{2}} b^{\frac{n+6}{2}} x^6 - \frac{1}{2^7} \times n \cdot \frac{n-10}{2} \cdot \frac{n-12}{3} \cdot \frac{n-14}{4} \cdot \frac{n-16}{5} \cdot \frac{n-18}{6} \cdot \frac{n-20}{7} \cdot \frac{n-22}{8} a^{\frac{n-24}{2}} b^{\frac{n+8}{2}} x^8 + \&c.) = a^n p + b^n p^3 \pm 2a^{\frac{n}{2}} b^{\frac{n}{2}} p^2$.

The quantities $\pm p^2$ and $\pm 2a^{\frac{n}{2}} b^{\frac{n}{2}} p^2$ denote $+p^2$, and $+2a^{\frac{n}{2}} b^{\frac{n}{2}} p^2$, if $\frac{n-2}{4}$ is a whole number, otherwise they denote $-p^2$ and $-2a^{\frac{n}{2}} b^{\frac{n}{2}} p^2$ respectively.

5. I. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an odd number, then

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then will $x^n - nabpx^{n-2} + n \cdot \frac{n-3}{2} a^2 b^2 p^2 x^{n-4} - n \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} a^3 b^3 p^3 x^{n-6} + n \cdot \frac{n-5}{2} \cdot \frac{n-6}{3} \cdot \frac{n-7}{4} a^4 b^4 p^4 x^{n-8} - \&c. = a^n p + b^n p^{n-1}$.

5. II. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an even number, then will $x^n - nabpx^{n-2} + n \cdot \frac{n-3}{2} a^2 b^2 p^2 x^{n-4} - n \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} a^3 b^3 p^3 x^{n-6} + n \cdot \frac{n-5}{2} \cdot \frac{n-6}{3} \cdot \frac{n-7}{4} a^4 b^4 p^4 x^{n-8} - \&c. = a^n p + 2a^{\frac{n}{2}} b^{\frac{n}{2}} p^{\frac{n}{2}} + b^n p^{n-1}$; it will be $+ 2a^{\frac{n}{2}} b^{\frac{n}{2}} p^{\frac{n}{2}}$ if $n = 4r + 2$; but $- 2a^{\frac{n}{2}} b^{\frac{n}{2}} p^{\frac{n}{2}}$ if $n = 4r$.

6. I. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-2}}$, and n an odd number, which has not the number 3 for a divisor, then will $x^n - na^2 b p x^{n-3} + n \cdot \frac{n-5}{2} a^4 b^2 p^2 x^{n-6} - n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^6 b^3 p^3 x^{n-9} + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^8 b^4 p^4 x^{n-12} - n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{10} b^5 p^5 x^{n-15} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{12} b^6 p^6 x^{n-18} - \&c.$ (to m terms, where m is the number either equal to, or the least greater than $\frac{n}{3}$) $- ab^{\frac{n+1}{2}} p^{\frac{n-1}{2}} (nx^{\frac{n-3}{2}} + \frac{1}{4}n \cdot \frac{n-5}{2} \cdot \frac{n-7}{3} a^2 b p x^{\frac{n-9}{2}} + \frac{1}{2^4}n \cdot \frac{n-7}{2} \cdot \frac{n-9}{3} \cdot \frac{n-11}{4} \cdot \frac{n-13}{5} a^4 b^2 p^2 x^{\frac{n-15}{2}} + \frac{1}{2^5} \times n \cdot \frac{n-9}{2} \cdot \frac{n-11}{3} \cdot \frac{n-13}{4} \cdot \frac{n-15}{5} \cdot \frac{n-17}{6} a^6 b^3 p^3 x^{\frac{n-21}{2}} + \frac{1}{2^6} \times n \cdot \frac{n-11}{2} \cdot \frac{n-13}{3} \cdot \frac{n-15}{4} \cdot \frac{n-17}{5} \cdot \frac{n-19}{6} \cdot \frac{n-21}{7} a^8 b^4 p^4 x^{\frac{n-27}{2}} + \frac{1}{2^7} \times n \cdot \frac{n-13}{2} \cdot \frac{n-15}{3} \cdot \frac{n-17}{4} \cdot \frac{n-19}{5} \cdot \frac{n-21}{6} \cdot \frac{n-23}{7} \cdot \frac{n-25}{8} a^{10} b^5 p^5 x^{\frac{n-33}{2}} + \&c.) = A = a^n p + b^n p^{n-2}$.

Let n be an odd number divisible by 3, then will the above-

above-mentioned quantity = $A = a^n p + b^n p^{n-2} + 3a^{\frac{n}{3}} b^{\frac{2n}{3}} p^{\frac{n}{3}-1} + 3a^{\frac{2n}{3}} b^{\frac{n}{3}} p^{\frac{n}{3}-1}$.

6. II. Let n be an even number, not dividible by 3, then will $x^n - na^2 b p x^{n-3} + n \cdot \frac{n-5}{2} a^4 b^2 p^2 x^{n-6} - n \cdot \frac{n-7}{2} \cdot \frac{n-8}{3} a^6 b^3 p^3 x^{n-9} + n \cdot \frac{n-9}{2} \cdot \frac{n-10}{3} \cdot \frac{n-11}{4} a^8 b^4 p^4 x^{n-12} - n \cdot \frac{n-11}{2} \cdot \frac{n-12}{3} \cdot \frac{n-13}{4} \cdot \frac{n-14}{5} a^{10} b^5 p^5 x^{n-15} + n \cdot \frac{n-13}{2} \cdot \frac{n-14}{3} \cdot \frac{n-15}{4} \cdot \frac{n-16}{5} \cdot \frac{n-17}{6} a^{12} b^6 p^6 x^{n-18} - \&c.$ to m terms as before $- b^{\frac{n}{2}} p^{\frac{n}{2}-1} (2x^{\frac{n}{2}} + \frac{1}{2} n \cdot \frac{n-4}{2} a^2 b p x^{\frac{n}{2}-3} + \frac{1}{2^3} \times n \cdot \frac{n-6}{2} \cdot \frac{n-8}{3} \cdot \frac{n-10}{4} a^4 b^2 p^2 x^{\frac{n}{2}-6} + \frac{1}{2^5} \times n \cdot \frac{n-8}{2} \cdot \frac{n-10}{3} \cdot \frac{n-12}{4} \cdot \frac{n-14}{5} \cdot \frac{n-16}{6} a^6 b^3 p^3 x^{\frac{n}{2}-9} + \frac{1}{2^7} \times n \cdot \frac{n-10}{2} \cdot \frac{n-12}{3} \cdot \frac{n-14}{4} \cdot \frac{n-16}{5} \cdot \frac{n-18}{6} \cdot \frac{n-20}{7} \cdot \frac{n-22}{8} a^8 b^4 p^4 x^{\frac{n}{2}-12} + \&c.) = A = a^n p - b^n p^{n-2}$.

Let n be an even number dividible by 3, then will the above-mentioned quantity $A = a^n p - b^n p^{n-2} - 3a^{\frac{2n}{3}} b^{\frac{n}{3}} p^{\frac{n}{3}-1} + 3a^{\frac{n}{3}} b^{\frac{2n}{3}} p^{\frac{n}{3}-1}$.

In all the preceding cases n , m and r denote whole affirmative numbers.

These equations may be deduced in the same manner as is before given in Case 3. I; or can be demonstrated by writing in the equation free from radicals for the different powers of x their values deduced from the given equation $x = a\sqrt[n]{p} + b\sqrt[n]{p}$.

To

To render the solution general, it may not be improper to subjoin the subsequent.

L E M M A.

1. Let $\alpha, \beta, \gamma, \delta, \varepsilon, \zeta, \&c.$ be the respective roots of the equation $x^n - 1 = 0$; then will $\alpha^m + \beta^m + \gamma^m + \delta^m + \varepsilon^m + \&c. = 0$, unless $n = m$, or n is a divisor of m , in which case $\alpha^m + \beta^m + \gamma^m + \delta^m + \varepsilon^m + \&c. = n$.

2. The sum of all quantities of the following kind $\alpha^m \beta^r + \alpha^r \beta^m + \alpha^m \gamma^r + \alpha^r \gamma^m + \beta^m \gamma^r + \beta^r \gamma^m + \alpha^m \delta^r + \&c.$ will be $= 0$; unless n be either equal to, or a divisor of $m + r$, in which case the sum above-mentioned will be $= -n$; except n be either equal to m or r , or a divisor of them, in which case the sum will be $n^2 - n$; but if $m = r$, then in the former case will the above-mentioned sum $= -\frac{n}{2}$, and in the latter $= \frac{n^2 - n}{2}$.

3. The sum of all quantities of this kind $\alpha^m \beta^r \gamma^s \delta^t \&c. + \alpha^r \beta^m \gamma^s \delta^t \&c. + \alpha^m \beta^r \gamma^t \delta^s \&c. + \alpha^r \beta^m \gamma^t \delta^s \&c. + \&c.$ will be $= 0$, unless n be either equal to $r + m + s + t \&c.$ or a divisor of it.

Let π be the number of indices $m, r, s, t, \&c.$ and n be either equal to $m + r + s + t + \&c.$ or a divisor of it, but n be neither equal to, nor a divisor of the sum of any two, three, four, ... $\pi - 3, \pi - 2$ or $\pi - 1$ of the above-mentioned

mentioned quantities; then will the sum above-mentioned $= \mp 1.2.3.4 \dots \pi-2.\pi-1 \times n$; where it will be +, if π be an odd number; otherwise -.

In this case, if a indices be m , b indices be r , c indices be s , d indices be t , &c. then will the above-mentioned sum $= \mp \frac{1.2.3.4 \dots \pi-2 \times \pi-1}{1.2.3 \dots a \times 1.2.3 \dots b \times 1.2.3 \dots c \times 1.2.3 \dots d \times \&c.} \times n$.

Let n be either equal to, or a divisor of the sum of any number ρ (less than π) of the above-mentioned quantities m, r, s, t , &c. and consequently either equal to, or a divisor of the sum of the $(\pi-\rho)$ remaining quantities: find the sum of all possible quantities of this kind $1.2.3 \dots \rho-2 \times \rho-1 \times 1.2.3 \dots \pi-\rho-2 \times \pi-\rho-1 \times n^2$, which sum call A.

Let n be either equal to, or a divisor of the sum of any number (σ) of the above-mentioned quantities m, r, s, t , &c; and also equal to, or a divisor of the sum of any number (ρ') of the remaining quantities, and consequently it will be either equal to, or a divisor of, the sum of the $(\pi-\rho'-\sigma)$ remaining quantities; then find the sum of all possible quantities of this sort $1.2.3 \dots \sigma-2 \times \sigma-1 \times 1.2.3 \dots \rho'-2 \times \rho'-1 \times 1.2.3 \dots \pi-\rho'-\sigma-2 \times \pi-\rho'-\sigma-1 \times n^3$, which sum call B.

In the same manner let n be either equal to, or a divisor of the sum of any number (τ) of the above-mentioned

tioned.

tioned quantities $m, r, s, t, \&c$; and similarly let n be either equal to, or a divisor of the sum of any number (τ) of the remaining quantities; and also let n be either equal to, or a divisor of the sum of any number (σ) of the remaining quantities; then will n be either equal to, or a divisor of the sum of the ($\pi - \tau - \sigma - \xi$) remaining quantities: find the sum of all quantities of this sort $1.2.3.. \tau - 2 \times \tau - 1 \times 1.2.3.. \sigma - 2 \times \sigma - 1 \times 1.2.3.. \xi - 2 \times \xi - 1 \times 1.2.3.. \pi - \tau - \sigma - \xi - 2 \times \pi - \tau - \sigma - \xi - 1 \times n^4$, which sum call c ; and so on; then will the above-mentioned sum $\alpha^m \beta^r \gamma^s \delta^t \&c. + \alpha^r \beta^m \gamma^s \delta^t \&c. + \alpha^m \beta^r \gamma^s \delta^t \&c. + \alpha^r \beta^s \gamma^m \delta^t \&c. + \&c. = \mp (1.2.3.. \pi - 2 \times \pi - 1 \times n - A + B - C + D - \&c.)$ where it will be $+$ if π be an odd number, otherwise $-$.

In this case, if a indices be m , b indices be r , c indices be s , d indices be t , $\&c$. then will the above-mentioned sum $= \mp \frac{1.2.3.. \pi - 2 \times \pi - 1 \times n - A + B - C + D - \&c.}{1.2.3.. a \times 1.2.3.. b \times 1.2.3.. c \times 1.2.3.. d \times \&c.}$.

7. Let $\alpha, \beta, \gamma, \delta, \epsilon, \&c$. be the roots of the equation $x^n - 1 = 0$, and the resolution be $x = a \sqrt[n]{p} + b \sqrt[n]{p^2} + c \sqrt[n]{p^3} + d \sqrt[n]{p^4} \dots + b \sqrt[n]{p^\lambda} \dots + k \sqrt[n]{p^\mu} \dots + l \sqrt[n]{p^\nu} \dots + q \sqrt[n]{p^\xi} \dots + r \sqrt[n]{p^\theta} \dots + s \sqrt[n]{p^{n-4}} + t \sqrt[n]{p^{n-3}} + v \sqrt[n]{p^{n-2}} + u \sqrt[n]{p^{n-1}}$; then will the different values of x be respectively $a \sqrt[n]{p} \times \alpha + b \sqrt[n]{p^2} \times \alpha^2 + c \sqrt[n]{p^3} \times \alpha^3 + d \sqrt[n]{p^4} \times \alpha^4 \dots + b \sqrt[n]{p^\lambda} \times \alpha^\lambda \dots + k \sqrt[n]{p^\mu} \times \alpha^\mu \dots + s \sqrt[n]{p^{n-4}} \times \alpha^{n-4} + t \sqrt[n]{p^{n-3}} \times \alpha^{n-3} + v \sqrt[n]{p^{n-2}} \times \alpha^{n-2} + u \sqrt[n]{p^{n-1}} \times \alpha^{n-1}$;

$a \sqrt[n]{p}$

$$a\sqrt[n]{p} \times \beta + b\sqrt[n]{p^2} \times \beta^2 + c\sqrt[n]{p^3} \times \beta^3 + d\sqrt[n]{p^4} \times \beta^4 \dots + b\sqrt[n]{p^{\lambda}} \times \beta^{\lambda} \dots + k\sqrt[n]{p^{\mu}} \times \beta^{\mu} \dots \dots + s\sqrt[n]{p^{n-4}} \times \beta^{n-4} + t\sqrt[n]{p^{n-3}} \times \beta^{n-3} + v\sqrt[n]{p^{n-2}} \times \beta^{n-2} + u\sqrt[n]{p^{n-1}} \times \beta^{n-1} ;$$

$$a\sqrt[n]{p} \times \gamma + b\sqrt[n]{p^2} \times \gamma^2 + c\sqrt[n]{p^3} \times \gamma^3 + d\sqrt[n]{p^4} \times \gamma^4 \dots + b\sqrt[n]{p^{\lambda}} \times \gamma^{\lambda} \dots + k\sqrt[n]{p^{\mu}} \times \gamma^{\mu} \dots \dots + s\sqrt[n]{p^{n-4}} \times \gamma^{n-4} + t\sqrt[n]{p^{n-3}} \times \gamma^{n-3} + v\sqrt[n]{p^{n-2}} \times \gamma^{n-2} + u\sqrt[n]{p^{n-1}} \times \gamma^{n-1} ;$$

$$a\sqrt[n]{p} \times \delta + b\sqrt[n]{p^2} \times \delta^2 + c\sqrt[n]{p^3} \times \delta^3 + d\sqrt[n]{p^4} \times \delta^4 \dots + b\sqrt[n]{p^{\lambda}} \times \delta^{\lambda} \dots + k\sqrt[n]{p^{\mu}} \times \delta^{\mu} \dots \dots + s\sqrt[n]{p^{n-4}} \times \delta^{n-4} + t\sqrt[n]{p^{n-3}} \times \delta^{n-3} + v\sqrt[n]{p^{n-2}} \times \delta^{n-2} + u\sqrt[n]{p^{n-1}} \times \delta^{n-1} ;$$

&c.

&c.

&c.

&c.

and consequently the sum of the values or roots, which is the coefficient of the second term of the equation

sought, will be $a\sqrt[n]{p} \times \alpha + \beta + \gamma + \delta + \&c. (o) + b\sqrt[n]{p^2} \times \alpha^2 + \beta^2 + \gamma^2 + \delta^2 + \&c. (o) + c\sqrt[n]{p^3} \times \alpha^3 + \beta^3 + \gamma^3 + \delta^3 + \&c. (o) + \dots + v\sqrt[n]{p^{n-2}} \times \alpha^{n-2} + \beta^{n-2} + \gamma^{n-2} + \delta^{n-2} + \&c. (o) + u\sqrt[n]{p^{n-1}} \times \alpha^{n-1} + \beta^{n-1} + \gamma^{n-1} + \delta^{n-1} + \&c. (o) = 0.$

The sum of the products of every two of the values or roots, which is the coefficient of the third term of the equation sought, will be $a^2\sqrt[n]{p^2} \times \alpha\beta + \alpha\gamma + \beta\gamma + \alpha\delta + \beta\delta + \gamma\delta + \&c. (o) + ab\sqrt[n]{p^3} \times \alpha\beta^2 + \beta\alpha^2 + \alpha\gamma^2 + \gamma\alpha^2 + \beta\gamma^2 + \gamma\beta^2 + \alpha\delta^2 + \delta\alpha^2 + \&c. (o)$, and in general all the terms will be 0, unless $a \times u \times p \times \alpha\beta^{n-1} + \beta\alpha^{n-1} + \alpha\gamma^{n-1} + \gamma\alpha^{n-1} + \beta\gamma^{n-1} +$

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$\gamma\beta$

$$\begin{aligned}
& \gamma\beta^{n-1} + \alpha\delta^{n-1} + \delta\alpha^{n-1} + \beta\delta^{n-1} + \delta\beta^{n-1} + \gamma\delta^{n-1} + \delta\gamma^{n-1} + \&zc. (-n) \\
& + b \times v \times p \times \alpha^2\beta^{n-2} + \beta^2\alpha^{n-2} + \alpha^2\gamma^{n-2} + \gamma^2\alpha^{n-2} + \beta^2\gamma^{n-2} + \gamma^2\beta^{n-2} \\
& + \alpha^2\delta^{n-2} + \delta^2\alpha^{n-2} + \beta^2\delta^{n-2} + \delta^2\beta^{n-2} + \&zc. (-n) + ct p \times \\
& \alpha^3\beta^{n-3} + \beta^3\alpha^{n-3} + \alpha^3\gamma^{n-3} + \gamma^3\alpha^{n-3} + \beta^3\gamma^{n-3} + \gamma^3\beta^{n-3} + \alpha^3\delta^{n-3} + \\
& \delta^3\alpha^{n-3} + \beta^3\delta^{n-3} + \delta^3\beta^{n-3} + \&zc. (-n) + dsp \times \alpha^4\beta^{n-4} + \beta^4\alpha^{n-4} + \\
& \alpha^4\gamma^{n-4} + \gamma^4\alpha^{n-4} + \beta^4\gamma^{n-4} + \gamma^4\beta^{n-4} + \&zc. (-n) + \&zc. = -np(au + \\
& bv + ct + ds + \&zc.)
\end{aligned}$$

If $n = 2\lambda$, then will the coefficient of b^2p be $\frac{n}{2}$, i. e. the above-mentioned coefficient will be $-np(au + bv + ct + ds + \dots + \frac{1}{2}b^2)$.

The sum of the contents of every three of the above-mentioned values or roots, which is the coefficient of the fourth term of the equation required, will be $\alpha^3\sqrt[3]{p^3} \times \alpha\beta\gamma + \alpha\beta\delta + \alpha\gamma\delta + \beta\gamma\delta + \&zc. (0) + a^2b\sqrt[3]{p^4} \times \alpha\beta\gamma^2 + \alpha\gamma\beta^2 + \beta\gamma\alpha^2 + \alpha\beta\delta^2 + \&zc. (0) + \&zc. + a^2v\sqrt[3]{p^n} \times \alpha\beta\gamma^{n-2} + \alpha\gamma\beta^{n-2} + \beta\gamma\alpha^{n-2} + \alpha\beta\delta^{n-2} + \alpha\delta\beta^{n-2} + \&zc. (\frac{1.2.n}{1.2}) + abt\sqrt[3]{p^n} \times \alpha\beta^2\gamma^{n-3} + \alpha\gamma^2\beta^{n-3} + \beta\alpha^2\gamma^{n-3} + \beta\gamma^2\alpha^{n-3} + \gamma\alpha^2\beta^{n-3} + \gamma\beta^2\alpha^{n-3} + \alpha\beta^2\delta^{n-3} + \&zc. (1.2.n) + \&zc.$; and in general all the terms (unless the quantity $\sqrt[3]{p^0}$ contained in the term have this formula $\sqrt[3]{p^n} = p$, or $\sqrt[3]{p^{2n}} = p^2$) will be $= 0$; let the general term be denoted by $bl\bar{k}\sqrt[3]{p^{\lambda+\mu+\nu}} \times \alpha^\lambda\beta^\mu\gamma^\nu + \alpha^\lambda\beta^\nu\gamma^\mu + \alpha^\mu\beta^\lambda\gamma^\nu + \alpha^\mu\beta^\nu\gamma^\lambda + \alpha^\nu\beta^\lambda\gamma^\mu + \alpha^\nu\beta^\mu\gamma^\lambda + \&zc.$ first let $\lambda + \mu + \nu$ neither be equal to n or $2n$, then will the

term

term above-mentioned = 0; if it be equal to n or $2n$, then will the term be $1.2 \times n \times bklp$ or $1.2nbklp^2$.

If two of the three indexes λ, μ, ν be equal to each other, then divide the above-mentioned term by 1.2 ; if the three indexes be equal, i. e. $\lambda = \mu = \nu$, divide it by $1.2.3$: find all quantities of this kind where $\lambda + \mu + \nu$ either is equal to n or $2n$, and add all the term from thence derived, and call the sum of them A .

The sum of the contents of every four of the values or roots above-mentioned, which is the coefficient of the fourth term of the equation required, will be $\frac{a^4 \sqrt{p^4} \times \alpha\beta\gamma\delta + \alpha\beta\gamma\delta + 8xc. (0) + a^3 b \sqrt{p^3} \times \alpha\beta\gamma\delta^2 + \alpha\beta\gamma^2\delta + \alpha\beta^2\gamma\delta + \alpha^2\beta\gamma\delta + 8xc. (0) + 8xc.}{bklq \sqrt{p^{4+\mu+\nu+\xi}} \times \alpha^\lambda\beta^\mu\gamma^\nu\delta^\xi + \alpha^\lambda\beta^\mu\gamma^\xi\delta^\nu + \alpha^\lambda\beta^\nu\gamma^\mu\delta^\xi + \alpha^\lambda\beta^\nu\gamma^\xi\delta^\mu + \alpha^\lambda\beta^\xi\gamma^\nu\delta^\mu + \alpha^\lambda\beta^\xi\gamma^\mu\delta^\nu + \alpha^\mu\beta^\nu\gamma^\xi\delta^\xi + 8xc.}$ denote a general term; this term will be = 0, unless $\lambda + \mu + \nu + \xi$ either = n or $2n$ or $3n$; in which case the term will be either $-1.2.3nbklqp$ or $-1.2.3nbklqp^2$ or $-1.2.3nbklqp^3$; unless $\lambda + \mu = \nu + \xi = n$, when the above-mentioned term will be $-(1.2.3n - n^2) bklqp^2$; in this case if $\lambda = \nu$, and consequently $\mu = \xi$, then it will be $-(1.2.3n - 1.2n^2) bklqp^2$; but if $\lambda = \mu = \nu = \xi = \frac{n}{2}$, then will the term be $-(1.2.3n - 3n^2) bklqp^2$.

In all these cases, if two of the indexes λ, μ, ν, ξ be equal, then must the term given above be divided by

0 2

1.2;

1.2; if three, by 1.2.3; if four, by 1.2.3.4; and lastly if two are equal to each other, and the two remaining indexes equal to each other, but not to the former two, then must the term aforesaid be divided by 1.2.1.2.

Find the sum of all the possible terms of this kind, which call B.

In the same manner from the preceding Lemma may be found the aggregates of the contents of every five, six, seven, &c. roots or values multiplied into each other, which call respectively c, d, e, &c; then will the equation required be $x^n - np(ax + bv + ct + ds + \&c.)x^{n-2} - Ax^{n-3} + Bx^{n-4} - Cx^{n-5} + Dx^{n-6} - \&c. = 0$.

From the same principles may be deduced the most general reduction yet known of equations to others of inferior dimensions, *e. g.*

Let (X) $x^n + (A + a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \dots + s\sqrt[n]{p^{n-2}} + t\sqrt[n]{p^{n-1}})x^{n-1} + (B + a'\sqrt[n]{p} + b'\sqrt[n]{p^2} + \dots + s'\sqrt[n]{p^{n-2}} + t'\sqrt[n]{p^{n-1}})x^{n-2} + (C + a''\sqrt[n]{p} + b''\sqrt[n]{p^2} + \&c.)x^{n-3} + \&c. = 0$; let $\alpha, \beta, \gamma, \delta, \&c.$ be the respective roots of the equation $z^n - 1 = 0$, then, from the principles before given, may be formed the different values of the equation X, which being multiplied into each other from the propositions before-mentioned of the *Meditationes Algebraicæ*, may be deduced an equation of *nm* dimensions free from radicals,

whose root is x , and which contains mn unknown quantities $A, a, b, c, \&c. B, d', b', c', \&c. C, a'', b'', c'', \&c. p$: for one, two or more of these unknown quantities may be assumed any quantities whatever, and thence may be deduced equations of mn dimensions, which may be reduced to equations $x^n + (A + a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \&c.) x^{n-1} + \&c. = 0$ of n dimensions.

In the same manner may be assumed equations, which involve $\sqrt[n]{p}, \sqrt[n]{p^2}, \dots, \sqrt[n]{p^{n-1}}; \sqrt[n]{q}, \sqrt[n]{q^2}, \sqrt[n]{q^3}, \dots, \sqrt[n]{q^{n-1}}; \sqrt[n]{r}, \sqrt[n]{r^2}, \sqrt[n]{r^3}, \dots, \sqrt[n]{r^{n-1}}, \&c$; and from so reducing them as to exterminate the irrational quantities, may often be derived equations whose resolutions or reductions are known.

The method of transforming algebraical equations into others, whose roots bear any assignable algebraical (but not exponential) relation to the roots of a given algebraical equation first published by me in the papers sent to the Royal Society, and afterwards in the year 1760; and thirdly in my *Miscellanea Analytica*; and lastly in the *Meditationes Algebraicæ*, and since published by Mr. LE GRANGE in the Berlin Acts, is perhaps (as Mr. LE GRANGE observes) more general than Mr. HUDDE's, or any transformation yet invented; it is very useful in the resolution of numerous problems; and further:

further has this peculiar advantage over all other transformations yet invented, that it often easily discovers some of the first terms of the equation required, from which many elegant Theorems may be derived.

In the works above-mentioned, viz. Miscell. Analyt. Medit. Algeb. &c. are given some problems serving to this transformation; the first of which is a series, which from the coefficients of a given algebraical equation ($x^n - px^{n-1} + qx^{n-2} - \&c. = 0$) finds the sum of any power of the roots (viz. $\alpha^n + \beta^n + \gamma^n + \delta^n + \&c.$ where $\alpha, \beta, \gamma, \delta, \&c.$ denote the roots of the given equation), the law of which series was published by me many years before that it was given by Mr. EULER. The third Problem often mentioned in this paper is an elegant and useful series for finding the sum of quantities of the following kind, viz. $\alpha^m \beta^r \gamma^s \delta^t, \&c. + \alpha^r \beta^m \gamma^s \delta^t, \&c. + \alpha^s \beta^m \gamma^r \delta^t, \&c. + \alpha^s \beta^r \gamma^m \delta^t, \&c. + \alpha^m \beta^r \gamma^s \delta^t, \&c. + \&c.$

Mr. EULER gave the following resolution; $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \&c.$ where $\pi, \rho, \sigma, \tau, \&c.$ denote the roots of an equation of $n-1$ dimensions $v^{n-1} - pv^{n-2} + qv^{n-3} - \&c. = 0$. It is evident, that in this case the equation whose root is x will have n^{n-1} dimensions; for let the roots of the equation $z^n - 1 = 0$ be denoted by $\alpha, \beta, \gamma, \delta, \&c.$ then will the quantity $\sqrt[n]{\pi}$ have the n following values

ures $\alpha\sqrt[n]{\pi}$, $\beta\sqrt[n]{\pi}$, $\gamma\sqrt[n]{\pi}$, &c. and the same may be affirmed of the quantities $\sqrt[n]{\rho}$, $\sqrt[n]{\sigma}$, $\sqrt[n]{\tau}$, &c. and consequently the quantity $\sqrt[n]{\pi} + \sqrt[n]{\rho}$ will have $n \times n$ different values; and in the same manner the root $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \dots$ may be proved to contain $n \times n \times n \times n \times \dots = n^{n-1}$ roots, and consequently in this resolution, in equations of superior dimensions, the number of independent coefficients ($n-1$) will be very few in proportion to the number of dimensions n^{n-1} , or (if we respect its formula) n^{n-1} of the resulting equation.

Let $n = 3$, and the equation resulting will rise to an equation of nine dimensions, which has the formula of a cubic; for let $x = \sqrt[3]{\pi} + \sqrt[3]{\rho} = a$ one root, then will $\frac{-1 + \sqrt{-3}}{2} a$ &c. $\frac{-1 - \sqrt{-3}}{2} a$ be two other of the nine roots, and consequently the roots will be $\overline{x^3 - a^3} \times \overline{x^3 - b^3} \times \overline{x^3 - c^3} = 0$, which has the formula of a cubic: and in general the above-mentioned equation of n^{n-1} dimensions will, for the same reason, have the formula of an equation of n^{n-1} dimensions.

Let the resolution be $x = \sqrt[n]{\pi} + \sqrt[n]{\rho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \dots$ where π, ρ, σ, τ , &c. denote the roots of an equation $x^{n-1} - px^{n-2} + qx^{n-3} - \dots = 0$ of $(n-1)$ dimensions, then will the resulting equation free from radicals, whose root is x , rise to 2^{n-1} dimensions; but as every affirmative:

tive has a negative root equal to it, it will have the formula of an equation of 2^{n-2} dimensions.

Let the resolution be of this formula $x = \sqrt[r \times m]{\alpha} + \sqrt[r \times m]{\beta} + \sqrt[r \times m]{\gamma} + \sqrt[r \times m]{\delta} + \&c.$ if $\alpha, \beta, \gamma, \delta, \&c.$ be considered as the r power of the roots of an equation of s dimensions, then will the resulting equation, of which the resolution is given, rise only to an equation of the formula of m^{s-1} dimensions.

In the year 1762 I published some reasons, for which this method could not extend to the general resolution of algebraical equations.

XI. *Observations on the total (with Duration) and annular Eclipse of the Sun, taken on the 24th of June, 1778, on Board the Espagne, being the Admiral's Ship of the Fleet of New Spain, in the Passage from the Azores towards Cape St. Vincent's. By Don Antonio Ulloa, F. R. S. Commander of the said Squadron; communicated by Samuel Horsley, LL.D. F. R. S.*

Read December 24, 1778.

THE public prints will have given notice of my arrival at this port, with the fleet under my command, on the 29th of June last. A very favourable, though

Observations de l'Eclipse du Soleil totale avec Retention et Annulaire faites le 24^e Juin 1778; sur le Vaisseau l'Espagne Commandant l'Escadre de la Flotte de la Nouvelle Espagne, en faisant le trajet des Isles Terceres vers le Cap St. Vincent. Par Don Antonio de Ulloa chef d'Escadre et Commandant General de la dite Flotte, Membre de la Societé Royale de Londres.

ON aura appris par les nouvelles publiques, que je suis rentré dans ce port le 29^e du mois passé avec la flotte de la Nouvelle Espagne sous mon commandement. Le trajet dans mon retour, qui a été long mais très heureux, m'a été favorable

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though long, passage gave me the opportunity of observing at sea the eclipse of the Sun, which was accompanied by a phenomenon observed by few astronomers, and which I myself saw for the first time; to wit, the luminous Annulus which surrounds the disk of the Moon: the following is a description of this appearance, one of the most beautiful that can be conceived.

The motion of the ship prevented our observing the beginning of the eclipse, by reason of the difficulty there was in keeping the Solar image and a part of that of the Moon within the field of the telescope; the object vanished every instant, and it was not till after several fruitless trials that I could get it again. Besides this, the arms grew tired of holding up the telescope and smoked glass, which could not be rested upon any thing from the necessity there was of moving the telescope in a contrary

table pour observer en mer l'Eclipse du soleil accompagnée d'un phenomene très particulier que j'ai vu, pour la première fois et que peu d'astronomes, ont observé, jusqu' à présent: c'est l'anneau lumineux autour du disque de la lune, phénomène des plus frapans et des plus beau à la vue; en voici la description:

Le mouvement du vaisseau ne permit pas d'observer le commencement de l'eclipse, par la difficulté de conserver le corps solaire et une partie de celui de la Lune dans le champ de la Lunette; l'objet m'échappoit à tout moment, et je ne le ratrappois qu'après bien des recherches. Outre cela, les bras se fatiguoient à soutenir en l'air la lunette et le verre obscur, qu'on ne pouvoit appuyer, parce qu'il falloit

trary direction to that of the ship. I had no other calculations but those to be met with in the *Connoissance des Temps*, which I did not find very exact, owing either to some error in the calculations themselves, or to the longitude of the ship's place not having been accurately determined in that book: I found a pretty sensible difference in the hour set down for the beginning of the eclipse. I observed the total obscurity of the Sun's disk at 44 minutes past three, the beginning of the emerfion at 48 minutes after three, the end of the eclipse at 48 minutes after four; consequently, the middle of it must have been at 46 minutes after three, or thereabouts: the total obscurity lasted four minutes, a sufficient time for observing the ring which was formed round the Moon.

Five

falloit faire avec la Lunette un mouvement contraire à celui du vaisseau. Je n'avois d'autres calculs que celui de la *Connoissance des Temps* que je ne trouvois pas de la dernière précision, soit qu'il se trouve quelque erreur dans le calcul, soit que la longitude estimée du lieu où le vaisseau setrouvoit ne fût pas la véritable. Je trouvois une différence assez sensible sur l'heure indiquée pour son commencement; J'observai la totale obscurité du disque du Soleil à 3 heures 44 min., le commencement de l'emerfion à 3 h. 48 m. la fin de l'eclipse à 4 h. 48 m. par conséquent le milieu devoit être à 3 h. 46 m. ou environ. La durée de l'eclipse totale du Soleil fut de 4 minutes, intervalle suffisant pour observer l'anneau qui se forma autour de la Lune.

Five or six seconds after the immersion we began to observe round the Moon a very brilliant circle of light, which seemed to have a rapid circular motion something similar to that of a rocket turning about its center. This light became livelier and more dazzling in proportion as the center of the Moon approached to that of the Sun, and about the middle of the eclipse it was of the breadth of about a sixth of the Moon's diameter. Out of this luminous circle there issued forth rays of light, which reached to the distance of a diameter of the Moon, sometimes more, sometimes less, which made me think that they were parts of a weaker light which were reflected in an atmosphere more subtle than that in which the ring was formed. When the centers of the two planets began to separate, the diminution began, and took

5 ou 6 secondes après que l'immersion eut été faite on commença à découvrir autour de la Lune un cercle de lumière très brillant qui sembloit avoir un mouvement rapide circulaire, semblable à celui d'un artifice embrasé mis en jeu sur son centre. Cette lumière devint plus vive et plus éblouissante à mesure que le centre de la Lune approchoit de celui du Soleil ; et dans le temps que l'eclipse fut à son milieu elle étoit large de deux doigts du diamètre de la Lune, ou comme la sixième partie du dit diamètre. De ce cercle lumineux partoient des rayons de lumière de toute sa circonférence, perceptibles jusqu'à la distance d'un diamètre de la Lune ; tantôt plus, tantôt moins, ce qui me fit penser que c'étoit des parties de lumière plus foibles qui s'imprimoient dans un atmosphère plus subtil que celui ou étoit formé l'anneau. Lorsque les centres des deux planètes commencèrent à s'écarter, la diminution commença et se fit graduellement dans le même

took place gradually in the same order which had been observed at its beginning and during the progress of it. It disappeared entirely four or five seconds before the emersion. The colour of the light was not the same every where; the part immediately joining the disk of the Moon was of a reddish cast, from thence it changed towards a pale yellow, which about the middle began to clear till, at the external extremity, it ended in an almost entire white. It was equally brilliant throughout, and the whirling motion, common to all the parts of it, seemed to change the form and position of the rays which appeared to the eye sometimes larger, sometimes shorter, at the same time that there was no change either in the colours of the ring themselves, or in the arrangement of them, both which continued as I have described them.

For

même ordre qui s'étoit observé dans son commencement et ses progrès : il disparut entièrement 4 ou 5 secondes avant l'emersion. La couleur de la lumière n'étoit pas la même partout : la partie immédiate au disque de la Lune étoit couleur rougeâtre, ensuite elle tiroit sur le jaune pâle ; et depuis le milieu jusqu'à l'extrémité extérieure cette couleur jaune s'éclaircissoit insensiblement jusqu'à tirer en fin tout à fait sur le blanc. Elle étoit également brillante partout ; et son mouvement de tourbillon, commun à toutes ses parties paroissoit changer la forme et la position des rayons en les présentant à la vue, tantôt plus courts, tantôt plus longs, sans cependant occasionner de changement dans les couleurs de l'anneau et leur arrangement qui restoit le même que je viens de détailler.

Quatre

For four or five seconds before the appearance of the shining ring, and during as many after it had disappeared, one could see the stars of the first and second magnitude as at the entrance of the night; but when it was in its greatest degree of brilliancy, only those of the first magnitude could be discovered. The darkness was such, that persons who were asleep, and happened to wake, thought that they had slept the whole evening, and only waked when the night was pretty far advanced. The fowls, birds, and other animals on board took their usual position for sleeping as if it had been night.

Before the edge of the Sun's disk emerged from that of the Moon, there was discovered near that of the latter a very small point of that of the Sun; it was imperceptible to the naked eye, but having looked at it
with

Quatre ou cinq secondes avant de voir paroître l'anneau brillant, et autant après sa suppression l'on vit, comme à l'entrée de la nuit, les étoiles de la 1^{re} et de la 2^{de} grandeur; mais lors qu'il étoit dans son brillant on ne voyoit que celles de la 1^{re}. L'obscurité fut au point que des personnes qui dormoient et qui s'éveillèrent crurent à leur grand étonnement d'avoir dormi toute la soirée et de ne s'être éveillées qu'assez avant dans la nuit. Les poules, les oiseaux et les autres animaux prirent leur position ordinaire pour dormir comme si c'eût été la nuit.

Avant que le bord du disque du Soleil parut par celui de la Lune, on découvrit près de celle-ci un très petit point de celui du Soleil imperceptible à la vue; mais l'ayant distingué par le secours de la lunette, je l'estimai d'abord de la
grandeur

with the glafs I estimated it at first to be about the magnitude of a star of the fourth order; after which it seemed to increase to that of one of the third. Its first appearance, to wit, that before the edge of the Sun emerged from that of the Moon, lasted about a minute and a quarter, the luminous circle was still visible though already much weaker than it had been.

The reddish colour of the ring towards the Lunar disk, its deep yellow towards the middle, its clear and very pale yellow at the external extremity, its uniform circumference, and the rays issuing from it to the distance taken notice of above, convince me that the whole is the effect of the Lunar atmosphere, which is of a substance different from that of the earth, that is, more transparent, more homogeneous, more uniform, and firmer

grandeur d'une étoile de la 4^e classe; et ensuite il me parut augmenter jusqu'à la grandeur de celles de la 3^e. Sa première apparition, c'est à dire avant que le bord du Soleil parut par celui de la Lune, fut de la durée d'environ une minute et un quart: le cercle lumineux subsistoit encore quoique déjà affoibli.

La couleur rougeâtre de l'anneau proche du disque de la Lune, jaunâtre comme couleur d'or vers le milieu, jaune clair et très affoibli vers sa partie extérieure; sa circonférence égale, et les rayons qui partent de cet anneau à la distance dite ci dessus persuadent que le tout est l'effet de l'atmosphère de la Lune, laquelle est de matière différente de celle de la terre; plus transparente, plus nette, plus égale et plus propre à réfléchir les rayons de lumière que celle ci; autrement, l'anneau n'auroit pas été également clair, brillant et coloré dans la circonférence entière du disque de la lune. On ne peut pas dire que cet anneau lumineux soit l'effet

ter for reflecting the rays of light, since otherwise the ring would not have been equally clear, shining, and coloured throughout the whole circumference of the Lunar disk. It cannot be said, that this luminous ring is the effect of the rays of the Sun reflected by the atmosphere of the earth, because the apparent diameter of the Sun is smaller than that of the Moon, whose disk entirely hid that of the Sun. Besides, if the luminous circle had been made by the atmosphere of the earth, its colours would have been like those of the rainbow, and it would have appeared fixed without motion, instead of which, that which was seen is the same as that which is seen by the naked eye upon the Sun when it is just above the horizon a little after Sun-rise or before Sun-set, so that one may conclude, that this luminous circle is a part of the disk of the Sun seen after refraction through the atmosphere of the Moon.

The

L'effet des rayons du Soleil réfléchis sur l'atmosphère de la Terre, puisque le diamètre apparent du Soleil est plus petit que celui de la Lune, dont le disque cachoit entièrement à nos regards ou à ceux du globe terrestre celui du Soleil. D'ailleurs, si c'étoit sur l'atmosphère de la Terre que le cercle lumineux fut formé, ce cercle auroit été semblable dans ses couleurs à l'arc-en-ciel, et il auroit paru fixe sans mouvement ; au lieu que celui qui a été aperçu est le même qu'on distingue dans le Soleil en le regardant directement avec la simple vue sur l'horizon peu après son lever ou un peu avant son coucher : en sorte qu'on peut conclure que ce cercle lumineux est une partie du disque du Soleil vu par refraction sur l'atmosphère de la Lune.

I

Le

The point of the Sun's disk, which was seen before its limb began to emerge from that of the Moon, is a very extraordinary phenomenon which I was not acquainted with before. In order to obviate all doubts which might arise about it, I must mention that we were three observers, Don JOACHIN D'ARANDA, Lieutenant WINTUYSEN, and I. Mr. D'ARANDA, who was looking at the eclipse through a two-foot telescope about the end of the total obscurity, was the first who perceived it. He, not knowing what it was, told me, that the total obscurity drew near an end, because he discovered a small point of the Sun, like a star, on the edge of the Moon. I looked immediately with the naked eye, and saw nothing. I then took out a spy glass, with which I saw as much. At length I took out my telescope of two feet and a half, and did discover with that a red luminous point so near the

Le point du disque du Soleil, vu avant que son limbe eusse commencé à paraître par celui de la Lune, est un phénomène très particulier et dont je n'avois pas connoissance. Pour prévenir les doutes qui peuvent se lever je dois dire que nous étions trois personnes à observer l'eclipse : M. M. Don JOACHIN DE ARANDA Cap. de frégate de l'armée, et pilote major de la flotte ; Don P. WINTUYSEN lieutenant de vaisseau et major de la dite flotte, et moi. Mr. D'ARANDA, en regardant l'eclipse vers la fin de la totale obscurité avec sa lunette de deux pieds, fut celui qui l'aperçut le premier : Il me dit, ne sachant ce que c'étoit, que la totale obscurité étoit près de finir, parce qu'il voyoit un petit point du Soleil sur le bord de la Lune semblable à une étoile. Je regardai d'abord avec la vue,

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et

the edge of the Moon, that it left me no doubts of its belonging to the body of the Sun. I, at that time, estimated it to be about the size of a star of the third magnitude; and imagine, that when Mr. D'ARANDA discovered it, it must have been like one of the fourth. This point gradually increased, and when it became of the bigness of a star of the second magnitude, the edge of the Sun emerged from that of the Moon. The interval between the first discovery of this point and the beginning of the emergence was about a minute and a quarter. This apparition of the Sun, before the beginning of the emergence, can only have taken place through some crevice or inequality on the limb of the Moon, not perceivable at the full Moon, by reason of the reflected rays which cross each other, and confuse it; whereas at the time of the eclipse,

et je n'aperçu rien : Je tirai une lorgnette de la poche et je n'en vis pas d'avantage ; enfin je pris ma Lunette de $2\frac{1}{2}$ pieds avec laquelle je découvris effectivement un point rouge et lumineux très proche du bord de la Lune qui ne me laissa aucun doute que ce ne fut le corps du Soleil. Je l'estimai pour lors comme une étoile de la 3^e grandeur, et je présume que lors que M. D'ARANDA la découvrit, elle pouvoit paroître comme une étoile de la 4^e grandeur. Ce point grandit successivement ; et lorsqu'il pouvoit être jugé comme une étoile de la 2^e grandeur le bord du Soleil parût par celui de la Lune. L'intervale qu'il y eut entre la 1^{re} vision de ce point et le commencement de l'émergence fut d'une minute et un quart. Cette apparition du Soleil avant le commencement de l'émergence ne peut avoir eu lieu qu'à travers de quelque fente ou inégalité qui se trouve sur le limbe de la Lune, et qui est imperceptible lors du plenilune à cause des rayons réfléchis qui se croisent et confondent la dite ouverture ; au lieu que dans le

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eclipse, the Moon's body being entirely obscured, the light of the Sun is behind, and comes through the smallest openings in the disk without any confusion.

The time elapsed between the first appearance of the Sun's body through the aperture of the limb of the Moon and the appearance of the limb of the Sun out of that of the Moon will serve to determine the depth of the said chink, aperture, or inequality, which is equal to the height of the eminencies which form it.

The luminous point was towards the North-west part of the Moon's disk, a little more to the North than the part of its limb through which that of the Sun appeared at the beginning of the emerfion; and it is remarkable, that no other luminous speck was perceived in the disk besides this. This aperture is therefore the only one in that part of the disk through which the emerfion was

to

tems de l'eclipse le corps de la Lune se trouvant entièrement obscurci, la lumière du Soleil se trouve par derrière et perce sans confusion par les plus petites ouvertures du disque de cet astre.

Le tems qui s'est écoulé depuis la première apparition du corps du Soleil par l'ouverture du limbe de la Lune jusqu'à l'apparition du limbe du Soleil par celui de la Lune servira à déterminer la profondeur de la ditte fente, ouverture, ou inégalité qui est égale à la hauteur des éminences qui la forment.

Le dit point lumineux étoit à la partie du Nord-Ouest du disque de la Lune, un peu plus au nord de l'endroit de son limbe par où se fit voir celui du Soleil au commencement de l'emerfion; et il est à remarquer qu'on n'aperçut pas d'autre, point lumineux dans le disque de la Lune que celui là: ainsi cette ouverture est

Q 2

unique

to begin; from whence one may be certain, that throughout the fourth part of the Moon's circumference, reaching from North to West, there is not any perceptible breaks in its limb besides that which was then observed. There can be no doubt but that the luminous speck which appeared through the aperture was part of the Sun's body; this is demonstrated, by the red fiery colour (the same as that which is seen when this luminary is looked at through a smoked glass) by its gradual increase, in proportion as the limb of the Sun came near that of the Moon, and in short by the colour, which at its emerging was just the same as that which had been seen through the opening.

It remains to be mentioned, that on the 24th day of July, the day on which I made the above observation

unique dans la partie du disque par où devoit commencer l'émerfion, et on peut affurer que dans la quatrième partie de la circonférence de la Lune, il n'y a pas dans son Limbe, depuis le Nord jufqu'à l'oueft d'autres fentes perceptibles que celle qui fut obfervée. Il n'y a point de doute que le point lumineux qui parut à travers l'ouverture ne fut le corps du Soleil; Cela eft démontré par la couleur de feu très rouge, la même qui fe voit quand on regarde cet Afre à travers d'un verre obfcurci, par la gradation qui eut lieu dans fon accroiffement à mefure que le Limbe du Soleil approchoit de celui de la Lune, et enfin, par la couleur du Soleil, qui lors qu'il déborda fut la même que celle qui s'étoit vue à travers de l'ouverture.

Il me refte à dire que le 24^e Juin, jour que je fis cette obfervation à bord du Vaiffeau

vation on board the *Espagne*, the aforesaid ship's latitude was $37^{\circ} 14''$ North, measured the same day; that since noon it had sailed direct East; that from the end of the eclipse to its being in the meridian of Cape St. Vincent it had sailed 301 sea miles East, making 100 sea leagues, reckoning 20 leagues to a degree, remains to be known the difference of meridians of the said Cape and the meridian of the different observatories of the capital cities of Europe, in order to determine the part of the sea in which the observation was made relatively to the observatories.

The solar spots were seen very distinctly both before and after the eclipse; there were six of them in all: two very near each other on the Eastern part of the disk; two towards the middle of the disk, likewise very near one another;

Vaifseau l'*Espagne* commandant l'escadre de la flotte de Vera Cruz, ce V^u étoit par 37 degrés 14 minutes de Latitude Septentrionale, observée le même jour; que depuis le Midy il avoit fait route à l'est directement; que depuis la fin de l'Eclipse jusqu'à être nord sud avec le Cap St. Vincent il a navigué à l'est 301 milles maritimes qui font 100 $\frac{1}{2}$ lieues maritimes de 20 au degré; reste à savoir la différence de Méridien entre le dit Cap et les différent Observatoires des villes capitales de l'Europe, pour déterminer l'endroit à la mer où l'observation fut faite relativement aux dits Observatoires.

Les taches du Soleil furent vues très distinctement avant et après l'Eclipse; elles étoient au nombre de six: deux à la partie de l'est du disque peu distantes l'une de l'autre: deux vers le milieu du disque aussi assez près l'une de l'autre:

CC

another; and two towards the North, verging towards the North-west.

The corrected altitude of the center of the Sun above the horizon, taken at the moment the eclipse ended, was $36^{\circ} 31''$. The atmosphere was very clear; the wind WNW, and not very strong. No clouds were visible, as is often the case at the sea, nor were there any till near fix, when there arose some just above the horizon.

The luminous ring made so strong an impression on me, as well from the beauty and brightness of its colour as from its rapid and uniform circular motion that I could neither reckon the stars visible in each interval from the total obscurity to the end of the eclipse, nor make several other observations on the colour and vivacity of their light.

et deux enfin vers la partie du Nord un peu vers Nordouest.

La hauteur corrigée du centre du Soleil sur l'horizon, prise au moment que l'Eclipse finit étoit 36 degres 31 m. L'atmosphère étoit très net et l'air de l'Ouest Nord-Ouest de moyenne force; on ne voyoit aucun nuage comme cela arrive souvent en mer; ce ne fut que vers les six heures qu'il s'en forma quelques uns sur l'horizon.

L'anneau lumineux me fit une impression si agreable tant par la beauté et l'eclat de sa couleur que par son mouvement circulaire uniforme et rapide que je ne pus ni compter les étoiles visibles dans chaque intervalle depuis la totale obscurité jusqu'à la fin, ni faire d'autres observations sur la couleur et la vivacité de leur lumière: Je m'attachai uniquement à l'anneau et ensuite au point lumineux

light. All I attended to was, first, the ring, and then the luminous speck which appeared through the disk of the Sun. These made the same impression on all who observed with me.

It is not easy to make celestial observations at sea with the same precision and delicacy as they are made at land, on account of the motion of the ship, and want of convenience to use the instruments. It would have been difficult (even if we had had a Micrometer) to have measured either the breadth of the ring, in order to see if it was equal every where, or the distance between the luminous speck seen on the disk of the Moon and its limb. I regret, however, exceedingly not having been able to make observations which would have been of such use in astronomy.

neux du Soleil au travers du disque de la Lune. Ce ravissement produisit le même effet sur ceux qui observoient avec moi.

Il n'est pas facile de faire en mer des Observations Celestes avec autant de précision et de délicatesse qu'on les fait à terre, à cause du mouvement du vaisseau et de la gêne à se servir des instrumens. Il auroit été difficile, quand même on auroit été pourvu d'un Micromètre, de mesurer la largeur de l'anneau pour examiner s'il étoit égal partout, comme aussi la distance du point lumineux vu sur le disque de la Lune jusqu'à son limbe. J'ai bien du regret de n'avoir pu faire ces observations qui auroient été d'un grand avantage pour la Physique des astres.



XII. Tentamen continens Theoriam Machinæ publicarum.

Autore Thoma Bugge, Astronomo Regio, Astron. et Mathem. Prof. in Academia Havniensi, e Societatibus Scient. Havniens. et Nidros. Communicated by Sir John Pringle, Bart. F. R. S.

Read Dec. 24, 1779.

INTER innumera commoda, quæ societati civili adfert Mechanica, haud minimum est ars publicas adigendi, seu, palos majores trabesque oblongas terræ impingendi. Artem hanc veteribus non fuisse ignotam ex pluribus VITRUVII locis potest probari. Etiam si celeberrimus hic antiquitatis autor machinam non describat, tamen extra omne dubium posita est veterum in hac arte peritia. Sine ea enim impossibile fuisset extruere pontes, molas, aggeres, pyramides, columnas, ædificia, quorum molem, majestatem, firmitatemque venerabundi admiramur et vix imitari audemus. Hæc omnia requirunt fortissimas et solidissimas substructiones. Si loca sint congestitia et palustria, publicæ machinarum vi adiguntur, tunc imponitur craticula; plures publicæ impinguntur, capitibus promi-

prominentibus: earum intercapedines opplenitur lapidibus maioribus, flicibus, arena majori, fossilia et mortario, quibus fundamentis demum superstruenda sunt aedificia.

Forma machinae, quae veteres architectones publicas adegerunt, non satis constat. Recentiores, varias si dederunt compages et configurationes. Complures descripserunt

LEOPOLD, DESAGULIERS, ET BELIDOR. Inter has eminent

et palmam omnibus praeripit publica machina VAU-

LOUIS inventa, a DESAGULIERS descripta, ac in usum per-

ducta dum fundamenta pontis Westmonasteriensis conji-

cerentur. Praecipua ejus commoda sunt, ut ad onus (quod

arietem vocare licet) elevandum minor requiratur homi-

num numerus, ut aries ad majorem elevatus altitudi-

nem libere decidat, utque arietem deciduum huius ma-

chinae iterum arripiat forceps, et mox flectat; quibus ma-

chinamentis brevissimo temporis spatio et paucissimis

operariis: maximus publicarum numerus ad maximam

profunditatem adigi possunt.

Theoriam effectus huius machinae quidem dedit BELI-

DOR. Sed, quantum video, eam superstruxit fundamento

prorsus erroneo et parum solido; eam deducit ex regulis

collisionis corporum, considerando publicam terrae im-

pingendam, et onus deciduum tanquam corpora collisa.

Quis autem non vidit regulam collisionis supponere mo-

ram liberum, et medium non tactum? Nullo signo ju-

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in dato casu, ubi sublicæ soli resistentia valde magna opponitur, applicari potest. Tentabimus aliam explicare hujus machinæ theoriā.

Res eo redit, ut onus ingens ex certa altitudine cadens percutiat sublicarum capita annulo ferreo cincta; considerabimus duas machinas; vocabimus onera cadentia = o et o ; altitudines, e quibus decidunt = A et a ; sublicarum adigendarum massas = M et m ; superficies earum, quoad terræ impactæ sunt = s et s ; et profunditates sublicarum in solo = p et p . Percursio oneris cadentis instar virium vivarum est æstimanda per productum ex massa oneris in quadratum celeritatis; hoc autem quadratum proportionale est altitudini, ex qua cadit onus; proinde percursio æstimari potest per factum ex massa oneris impingentis in altitudinem computatam a supremo puncto ad caput sublicæ. Sed effectus sunt uti vires causarum suarum plenæ. Ergo, si resistentiam soli et massas sublicarum utrobique æquales statuamus, profunditates, ad quas singulis percursionibus adiguntur sublicæ, erunt in ratione composita e rationibus directis onerum et altitudinum; seu

$$p : p = a \times o : A \times o.$$

Si sumamus coherentiā soli æqualem et homogēnam, resistentia, quam, dum subsident, vincere debent sublicæ, crescit in ratione superficialium solo impactarum. Si

XIX. Jam

jā statuamus onera cadentia æqualia $o = o$; altitudines quoque æquales $A = a$; patet effectus percussionum et hinc profunditates decrescere prout crescunt tam superficies adactæ quam publicarum pondera vel massæ. Hinc sub data hypothese, erunt profunditates in ratione composita e rationibus inversis superficierum et massarum, seu

$$p : P = s \times M : f \times m = \frac{1}{f \times m} : \frac{1}{s \times M}.$$

Si jam omnia sunt inæqualia, nempe onera cadentia, altitudines, massæ publicarum, et earum superficies in terra conditæ; dico profunditates, singulo ictu acquisitas, esse in ratione composita e rationibus directis onerum cadentium et altitudinum, et e rationibus inversis superficierum et massarum. Concipiamus tertiam publicam, cujus massa = m , superficies adacta = s ; onus impingens = o ; altitudo, e qua cadit onus = a ; dicatur profunditas hujus publicæ ex data percussione proveniens = π . Tunc erit per antea demonstrata

$$p : \pi = \frac{1}{\pi \times f} : \frac{1}{m \times s}.$$

$$\pi : P = a \times o : A \times O.$$

$$\text{Ergo } p : P = \frac{a \times o}{m \times f} : \frac{A \times O}{m \times s}.$$

Hæc theoremata inserviunt diversis publicarum machinis comparandis, praxique exercendæ. Ad determinandum

R 3 sumtus.

sumtus operis extruendi maxima sunt utilia; Neque tamen
perfruiimus sequens problema.

*Calculo definire profunditatem ad quam singulo ictu sub-
sudet publica data machine vi usata.*

Cum in hoc casu tam onus cadens quam massa pali
impingendi constans sit et æqualis; erit $o = o$, et $M = m$.

Hinc explicata proportio fundamentalis in sequentem
abit.

$$p : p = s : s$$

Porro superficies tubularum terre impactarum sunt
rectangula eandem bati sed diversam altitudinem p et
 p habentia (p hic significat profunditatem totalem).

Quapropter $s : s = p : p$, id quod simpliciore et commo-
diorem subministrat analogiam.

$$p : p = s : s$$

Post percurssiones quasdam factas, adeo ut firmiter terræ
inhæreat publica, fiat denique novus ictus, quem pro

primo numeramus, tum cadat onus ex altitudine $= a$ et

subsideat publica profunditate $= p$. Fiat tum secundus
ictus, tunc subsidebit palus profunditate $= x$. Onus cadet

per altitudinem $A = a + p$, et publice profunditas totalis
erit $= p + x$. Facta jam debita substitutione habemus

$$p : x = \frac{a}{p} : \frac{a+p}{p+x}$$

Ex

Ex qua analogia originem ducit sequens æquatio.

$$px + x^2 = p^2 \frac{p^2}{2} = x + x_0$$

Quæ æquatio quadratica, si resolvanur, dabit valorem incognitæ

Applicemus calculum ad datum exemplum. Sit altitudo, a qua decidit onus percutiens, $a = 3$ ped = 36 pol.

Profunditas ad quam primo ictu subfidet publica = 4 pol. dicatur jam profunditas ad quam secundo ictu

subsidebit x , erit: non audiret si istæq. nullæ inveniatur

et præcedenti

utq. audiret

nonq. audiret

Secundo ictu publica subfidet numero rotundo 3 pol. In

tertio ictu erit altitudo, quam percurrit onus impingens = 36 + 4 + 3 = 43 pol. Profunditas publicæ = 4 + 3 = 7

pol. Denique profunditas tertio ictu acquisita = x ; tunc,

subnoxia hæc $4 : x = \frac{36}{4} : \frac{43}{4}$

ad se illudineq. quæ $x = 2$ pol. quam proxime.

In quarto ictu est altitudo = 36 + 4 + 3 + 2 = 45 pol. et profunditas publicæ = 4 + 3 + 2 = 9 pol. Profunditas quarto

ictu acquisita = x ; tunc erit,

$$4 : x$$

$$4 : x = \frac{36}{4} : \frac{45}{9+x}$$

$$9x + x^2 = \frac{180}{9}$$

$$x = \frac{12 \pm \sqrt{144 - 180}}{2} = 1 \frac{1}{2} \text{ pol. quam proxime.}$$

Idem problema sibi solvendum sumsit D. BELIDOR. Supponit publicam primo ictu subsidere 15 pol. Tunc juxta ejus calculos secundo ictu subsidebit 17; tertio 19; quarto 21; quinto 23. Quis autem non vidit hanc progressionem cum theoria et experientia pugnare? Profunditas enim singulo ictu acquisita continuo decrescit, et tandem publica repetitis ictibus non amplius subsidet. Id quod evenit, quando cohærentia soli et frictio major est vi a percussione oriunda. Eo in casu publica profundius adigi non potest nisi augeantur vel onus percussiens vel altitudo, e qua cadit.

In actis Academiæ Stockholmiensis mechanicus quidam haud incelebris statuit publicam ponderibus oneratam in hoc casu ulterius et profundius in terram adigi posse. Verum si publica ita oneratur, idem est ac si publicæ massa major esset, quod non auget sed impedit effectum percussione. Ad adigendam publicam pondus impositum agit sola pressione, quæ pro insensibili est habenda respectu resistentiæ et frictionis. Si vero onus impingens augeatur, certissime quoque augebitur percussio. Quam rem theoria et experientia confirmant; notum enim

enim est ex experimentis. D. CAMUS malleum 12 librarum ambabus manibus elevatum et proinde ex altitudine 5 pedum cadentem eundem effectum reddere ac simplex pressio 1000 librarum.

Determinare maximam profunditatem, ad quam publica data machina datâ adigi potest.

Sit altitudo, quam onus percurrit in primo ictu = a ; profunditas per primum ictum acquisita = p ; post factas complures percussiones publica subidet quantitate admodum parva = m ; et tum operæ pretium non est plures dare percussiones. Profunditas totalis antea acquisita = x ; et altitudo, quam onus percutiens, describit = $a + x$. Hinc,

$$p : m :: \frac{a}{p} : \frac{a+x}{x}$$

Ex resolutione hujus æquationis invenitur,

$$x = \frac{ap^2}{a^2 - p^2}$$

Sit $p = 4$ pol.; $m = \frac{1}{16}$ pol.; $a = 36$ pol.; invenitur maxima profunditas $x = \frac{36 \times 16}{144 - 16} = 576 : \frac{124}{10} = 46,6$ pol. Hæc

quantitas negativa esse debet, cum opposita sit altitudini, quæ instar positivæ est assumpta.

Corodinis loco sequentes practicas observationes adjungimus. 1. Ponderus oneris impingentis ut plurimum

est 800 libr. Vis hominis onus continuo labore ele-

vantis

vantis circiter 40 lib. æstimanda est. Hinc in machina
 simpliciter 20 hominesq. onus elevare valent. Ictibus autem
 23 vel 30 statim, ut per æquale temporis spatium requi-
 escant necesse est. 2. Ad percussiones 30 dandas et re-
 quem capiendam 4 minuta prima requiruntur, adeo
 ut per integram horam 450 ictibus publicam percu-
 tere liceat. 3. Viriam impedimentum est, si diameter
 tracheæ superioris, cui funis onus elevans circumvol-
 vitur sit justo minor. 4. Si onus impingens ope ma-
 norum et non mediantes axi in peritrochio est elevandum,
 altitudo, quam percussit onus, non superare potest 5
 pedes quos solummodo emetiri possunt homines brachio-
 rum libero motu. 5. Quando plures adigendæ sunt sub-
 licæ, prodest opus incipere a medio, et ad extremi-
 tates procedere, si enim contrario modo rem aggressus
 fueris, sublicæ intermediæ adigi non possunt sine opere
 et temporis dispendio propter soli compactionem. 6.
 Antequam opus incipiatur, terra est examinanda et sub-
 lica probatoria adigenda, ut exactam de soli qualitate
 ejusque fructus habeamus cognitionem. 7. His presupposi-
 tis, æstimari possunt summe ad seriem sublicarum adig-
 endam necessaria. 8. Ad machinam transferendam et sin-
 gulas sublicas perpendiculariter erigendas requiruntur 15
 minuta prima. 9. Ictibus probatoriis juxta præcedentia
 citata 6

pro-

problemata calculari possunt numerus percussionum per singula strata et hinc tempus totum requisitum. Tandem ex longitudine operis, ex pretio et numero publicarum aestimari possunt sumtus omnes quos impendere oportet.

Havna, 30 Augusti 1778.

**XIII. Account of an Iconantidiptic Telescope, invented by
Mr. Jeurat, of the Academy of Sciences of Paris. Com-
municated by John Hyacinth de Magellans, F. R. S.**

Read January 19, 1778.

MR. JEAURAT, of the Royal Academy of Sciences of Paris, having discovered a construction of the Iconantidiptic Telescope, thought proper to communicate to the Royal Society of London a short description of this new invention.

This Telescope is called the Iconantidiptic Heliometer, because it produces two images of the objects, the one in a direct position, and the other reversed. These two images,

Construction d'une lunette Iconantidiptique inventée par Mr. Jeurat, de l'Academie Royale des Sciences de Paris.

M• JEAURAT, de l'Academie Royale de Paris, ayant imaginé une construction de lunette Iconantidiptique, croit devoir communiquer à la Société Royale de Londres l'exposition succinte de cette nouvelle invention.

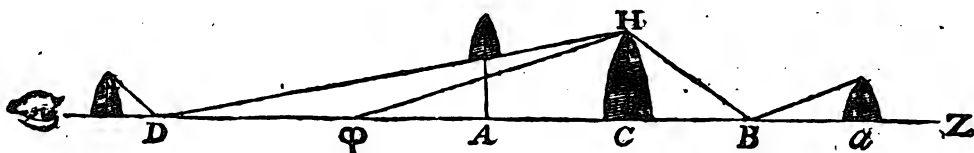
La lunette est appelée Heliometre Iconantidiptique parce qu'elle represente deux images des objets, l'une dans une situation droite et l'autre dans une situa-
tion

images, of opposite situation to each other, are exactly of the same size, and produce the effect of shewing the stars as entering at once both on the right and left sides of the Telescope. The first coincidence of the two images on the side of each other gives the passage of the first limb; the exact coincidence of the two images upon one another gives the passage of the center of the star; and the last coincidence of the two images at the side of each other gives the passage of the second edge: from whence it follows, that we not only observe as usual the passage of the two sides of the disk of a star, but also the direct passage of the center of the star: an observation which could not before be made in a direct manner. Besides, it may be observed, that this invention obviates the difficulty of illuminating the threads of the Telescope in observing very

tion renversée. Ces deux images de situation opposée l'une à l'autre sont exactement de la même grandeur et produisent l'effet de voir tout à la fois entrer les astres par la droite et par la gauche de la lunette. La première coincidence des deux images à côté l'une de l'autre donne le passage du premier bord, la coincidence exacte des deux images l'une sur l'autre donne le passage du centre de l'astre, et la dernière coincidence des deux images l'une à côté de l'autre donne le passage du second bord, d'où il suit que non-seulement on observe comme à l'ordinaire le passage des deux bords du disque d'un astre, mais aussi le passage direct du centre de l'astre: observation qui n'a jusqu'à présent pu se faire d'une manière directe. D'ailleurs on remarquera que cette invention supplée à la difficulté d'éclairer les fils de la lunette lorsqu'il est question d'observer de très petites étoiles, car avec cette

very small stars, for in this construction there is no occasion to see the threads.

The following is the construction of this Iconanti-diptic Telescope, which I have already made use of, and which appears to be proper for observations made in the plane of the Meridian.



That the solution may be applicable to Telescopes, it is proper that $AD=AZ$, $aB=az$.

Then put $AD=F$ the focal distance of the lens A ,

$aB=f$ the focal distance of the lens a ,

$aA=F-f$,

$AB=aA-aB=F-2f$,

BC

construction on n'a pas besoin de voir les fils.

Voici la construction de cette lunette Iconantidypique dont je me suis déjà servi et qui me paroît commode pour les observation faites dans le plan du Meridien.

Pour que la solution soit aussi applicable aux oculaires il convient que $AD=AZ$, $aB=az$.

Alors on fera $AD=F$ foyer de la lentille A ,

$aB=f$ foyer de la lentille a ,

$aA=F-f$,

$AB=aA-aB=F-2f$,

$$BC = x,$$

$$CD = y,$$

ϕ the focal distance of the lens c.

$$\text{Hence } \begin{cases} BD = AD + AB = 2 \times F - f, \\ BD = BC + CD = x + y. \end{cases}$$

The two values of BD evidently give, 1st, $x + y = 2 \times F - f$.

That the image B, given by the lens a, be seen at the distance BC: and that the direction of the ray BHD may form a relative focus in D, whose distance may be equivalent to AD, it is necessary that $aB \times CD = AD \times BC$; namely, 2^{dly}, $fy = Fx$.

That the object B, seen in the direction BH, may form a focus in D, it is necessary that the focal distance of the lens.

$$bc = x,$$

$$cd = y,$$

ϕ foyer de la lentille c.

$$\begin{cases} BD = AD + AB = 2(F - f), \\ BD = BC + CD = x + y. \end{cases}$$

Les deux valeurs de BD donnent évidemment, 1^o, $x + y = 2(F - f)$.

Pour que l'image B, donnée par la lentille a, soit vue à la distance BC: et pour que la direction du rayon BHD forme un foyer relatif en D, dont le foyer équivalent soit AD; il faut (voyez l'optique de SMITH de la Traduction de l'Abbé ROCHON, p. 278. art. 245.) dis-je que $aB \times CD = AD \times BC$ savoir, 2^o, $fy = Fx$.

Pour que l'objet B vu selon la direction BH, forme un foyer en D, il faut que

lens c (namely, the distance ϕ) have this condition,
 $\phi \times BD = BC \times CD$, viz. 3dly, $2\phi \times F - f = xy$.

From these three conditions, viz. $\left\{ \begin{array}{l} 1^\circ \quad x + y = 2 \times F - f, \\ 2^\circ \quad fy = Fx, \\ 3^\circ \quad 2\phi \times F - f = xy, \end{array} \right\}$ we easily and incon-
 testably find what follows:

$BC = x = \frac{2f \times F - f}{F + f}$ the dist. from the focus B to the lens CH,

$CD = y = \frac{2F \times F - f}{F + f}$ the dist. of the relative focus D, with re-
 spect to the two lenses a and c,

$\phi = \frac{2Ff \times F - f}{(F + f)^2}$ the focal distance of the lens c.

This solution just found is general; but to adapt it to a particular case, which may be proper for practice, I shall investigate what relation ought to take place between the distances F and f when ϕ is $= f$. This supposition

le foyer de la lentille c (avoir le foyer ϕ) ait cette condition ci $\phi \times BD = BC \times CD$
 savoir, 3°, $2\phi(F - f) = xy$.

Avec ces trois conditions, savoir, $\left\{ \begin{array}{l} 1^\circ \quad x + y = 2(F - f), \\ 2^\circ \quad fy = Fx, \\ 3^\circ \quad 2\phi(F - f) = xy, \end{array} \right\}$ on trouve facile et incontestablement ce qui suit.

$BC = x = \frac{2f(F - f)}{F + f}$ distance du foyer B à la lentille CH.

$CD = y = \frac{2F(F - f)}{F + f}$ le foyer relatif D, à l'égard des deux lentilles a et c.

$\phi = \frac{2Ff(F - f)}{(F + f)^2}$ foyer de la lentille c.

Cette solution qu'on vient de trouver est generale. Mais afin d'adopter un cas particulier, qui soit commode à pratiquer, je vais chercher les rapports, que doivent

fition gives $\phi = \frac{2Ff \times F - f}{F - f} = f$; from which we easily and incontestably extract the relation sought, *videlicet*, $F = \sqrt{5 + 2} \times f$; or this, which comes to the same thing, $f = (\sqrt{5 - 2}) \times F$.

But $\left\{ \begin{array}{l} \sqrt{5 + 2} = 4,2361 \\ \sqrt{5 - 2} = 0,2361 \end{array} \right\}$ Therefore for the case in which $\phi = f$, we have $\left\{ \begin{array}{l} F = 4,2361 f \\ f = 0,2361 F \end{array} \right\}$ The relation of the focal diff.

The

doivent avoir entre eux les foyers F et f dans le cas où l'on auroit $\phi = f$. Cette supposition donne $\phi = \frac{2Ff(F - f)}{(F + f)^2} = f$; d'où l'on tire facilement et incontestablement le rapport cherché que voici $F = (\sqrt{5 + 2})f$; ou bien celui ci qui revient au même que le précédent $f = (\sqrt{5 - 2})F$.

Mais $\left\{ \begin{array}{l} \sqrt{5 + 2} = 4,2361 \\ \sqrt{5 - 2} = 0,2361 \end{array} \right\}$ donc pour les cas $\left\{ \begin{array}{l} F = 4,2361 f \\ f = 0,2361 F \end{array} \right\}$ rapport des foyers.

Application

The Application of the general Formula to the particular case of the equal lenses a and c.

Let $AD = F$, the focal distance of the lens A,

The relation $f = 0,2361 F$, found for the focal distances, gives

$$\left\{ \begin{array}{l} AB = f = 0,2361 F, \text{ the focal dist. of the lenses } a \text{ and } c, \\ AA = F - f = 0,7639 F, \text{ the dist. between these lenses,} \\ AB = F - 2f = 0,5278 F, \text{ distance,} \\ BD = 2 \times F - f = 1,5278 F, \text{ distance,} \\ BC = \frac{2f \times F - f}{F + f} = 0,2918 F, \text{ distance,} \\ CD = \frac{2F \times F - f}{F + f} = 1,2360 F, \text{ distance.} \end{array} \right.$$

The

Application de la formule generale au cas particulier des lentilles egales a and c.

Soit $AD = F$, foyer de la lentille A,

Le rapport trouvé des foyers $f = 0,2361 F$, donne

$$\left\{ \begin{array}{l} AB = f = 0,2361 F, \text{ foyer des lentilles } a \text{ and } c. \\ AA = F - f = 0,7639 F, \text{ distance entre ces lentilles,} \\ AB = F - 2f = 0,5278 F, \text{ distance,} \\ BD = 2(F - f) = 1,5278 F, \text{ distance,} \\ BC = \frac{2f(F - f)}{F + f} = 0,2918 F, \text{ distance,} \\ CD = \frac{2F(F - f)}{F + f} = 1,2360 F, \text{ distance.} \end{array} \right.$$

Application

The numerical application of the particular case of the equal lenses a and c .

		Ft.	In.
Let	$AD=F=1728$ lines,	=	12 0
Then we shall have	$\left\{ \begin{array}{l} AB=0,2361 F=408 \text{ lines,} \\ AB=0,5278 F=912 \text{ lines,} \\ AA=0,7639 F=1320 \text{ lines,} \\ AD=AB+BD=1,7639 F=3048 \text{ lines,} \end{array} \right.$		$\left\{ \begin{array}{l} 2 \text{ 10} \\ 6 \text{ 4} \\ 9 \text{ 2} \\ 21 \text{ 2} \end{array} \right.$

It is from this particular case, in which $\phi=f$, and $f=F \times \sqrt{5-2}=0,23607 F$, that the following table is constructed.

Application numerique du cas particulier des lentilles égales a et c .

		P.	P.
Soit	$AD=F=1728$ lignes	=	12 0
Alors on aura	$\left\{ \begin{array}{l} AB=0,2361 F=408 \text{ lignes,} \\ AB=0,5278 F=912 \text{ lignes,} \\ AA=0,7639 F=1320 \text{ lignes,} \\ AD=AB+BD=1,7639 F=3048 \text{ lignes,} \end{array} \right.$		$\left\{ \begin{array}{l} 2 \text{ 10} \\ 6 \text{ 4} \\ 9 \text{ 2} \\ 21 \text{ 2} \end{array} \right.$

C'est d'après ce cas particulier, où l'on suppose $\phi=f$, et que $f=F(\sqrt{5-2})=0,23607 F$, qu'est construite la table qui suit.

Focal distance of the lens A and the equivalent AD.	Focal distances of the lenses a and c; and distance CA from the 2d lens c to the third a.	Distance ac from the first lens a to the second c.	Distance aa from the first lens a to the third A.	Whole distance ad.	Distance cd from the 2d lens c to the focus D.
Fr. ft. In.	Fr. ft. In. Lin.	Fr. ft. In. Lin.	Fr. ft. In. Lin.	Fr. ft. In. Lin.	Fr. ft. In. Lin.
0 1	0 0 3	0 0 6	0 0 9	0 1 9	0 1 3
0 2	0 0 6	0 1 1	0 1 6	0 3 6	0 2 6
0 3	0 0 8	0 1 7	0 2 3	0 5 3	0 3 8
0 4	0 0 11	0 2 1	0 3 1	0 7 1	0 4 11
0 5	0 1 2	0 2 8	0 3 10	0 8 10	0 6 2
0 6	0 1 5	0 3 2	0 4 7	0 10 7	0 7 5
0 7	0 1 8	0 3 8	0 5 4	1 0 4	0 8 8
0 8	0 1 11	0 4 3	0 6 1	1 2 1	0 9 11
0 9	0 2 1½	0 4 9	0 6 10½	1 3 10½	0 11 1
0 10	0 2 4½	0 5 3	0 7 8	1 5 8	1 0 4
0 11	0 2 7	0 5 10	0 8 5	1 7 5	1 1 7
1 0	0 2 10	0 6 4	0 9 2	1 9 2	1 2 10
2 0	0 5 8	1 0 8	1 6 4	3 6 4	2 5 8
3 0	0 8 6	1 7 0	2 3 6	5 3 6	3 8 6
4 0	0 11 4	2 1 4	3 0 8	7 0 8	4 11 4
5 0	1 2 2	2 7 8	3 9 10	8 9 10	6 2 2
6 0	1 5 0	3 2 0	4 7 0	10 7 0	7 5 0
7 0	1 7 10	3 8 4	5 4 2	12 4 2	8 7 10
8 0	1 10 8	4 2 8	6 1 4	14 1 4	9 10 8
9 0	2 1 6	4 9 0	6 10 6	15 10 6	11 1 6
10 0	2 4 4	5 3 4	7 7 8	17 7 8	12 4 4
11 0	2 7 2	5 9 8	8 4 10	19 4 10	13 7 2
12 0	2 10 0	6 4 0	9 2 0	21 2 0	14 10 0



XIV. Account of the Organs of Speech of the Orang Outang. By Peter Camper, M. D. late Professor of Anatomy, &c. in the University of Groningen, and F. R. S. in a Letter to Sir John Pringle, F. R. S.

Read January 7, 1779.

S I R,

Klein-Lankum, near Franeker,
Dec. 2, 1778.

BEING lately informed by Dr. POEMMERING, whom on account of his singular industry and talents I have recommended to your favour, that you, as well as l'Abbé FONTANA and Dr. INGENHOUSZ, were surprized to hear from M. FEBRONI, the keeper of the Duke of Tuscany's Museum; that I had discovered the true organical reason for which the Orang Outang, and several other apes and monkeys, are unable to speak; I take the liberty of addressing to you this anatomical essay upon the organ of speech of the Orang Outang and other monkeys, in hopes you will judge it worthy to be read to the Royal Society; in whose most valuable Transactions I should be very proud to see these observations; the rather, as it is the first essay I have offered to that respectable body.

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It is this discovery (of which I have already given a hint at the end of my Observations on the Rein-Deer) which Professor ALLAMAND has inserted amongst his additions to the XVth volume of the Count DE BUFFON'S Nat. Hist. printed at Amsterdam 1771, for S. H. SCHNEIDER, p. 55, 56. Professor ALLAMAND has moreover added, in the same volume, p. 76: *M. le Prof. CAMPER a observé les organes de la voix de l'Orang; il s'est convaincu qu'il étoit impossible, qu'il formât des tons articulés, comme les hommes en peuvent former, &c.*

It is asserted by a great many travellers, that though the Orang Outang does not speak, he would be able to articulate if he chose it. Several Naturalists seem to leave this question undetermined, from not having had the opportunity of dissecting this very uncommon animal; others again overlook it, being deeply engaged in the researches of other parts.

My principal study in this essay will be to prove the absolute impossibility there is for the Orang and other monkeys to speak.

§ 1. Being Professor of Natural Philosophy, Anatomy, Surgery, and Physic, in the University of Franeker in Friesland, I soon perceived the impossibility of understanding the most precious and valuable works of the immortal GALEN (especially his anatomical works) without

without dissecting monkies, to compare his exact descriptions with. I got for that purpose, in the year 1754, a *Cynocephalus*, and was charmed to find the exactness of almost all GALEN's descriptions. The organ of speech puzzled me, nevertheless, very much, and I was not able to explain his observations, so as to satisfy myself in this animal: I was obliged, therefore, for want of other apes, to delay my researches to another opportunity, which, however, I did not meet with till I came to Amsterdam, where I settled in the year 1755.

I discovered, at the beginning of 1757, in another *Cynocephalus*, that the basis of the *os hyoides* was very large and hollow; and that a membranous bag, lying under the *latissimi colli* (which touch one another in the middle of the neck in these animals) went up into this bony cavity, having a communication with the inside of the *larynx* by a hole at the root of the *epiglottis*.

The structure of these and the Orang will be better understood by the four figures engraved after my own drawings for the anatomy of the Orang. I intend to publish them with that of the Rhinoceros with the double horn, and the Rein-Deer.

In the *Cynocephalus* I found the whole organ of voice pretty much like that of dogs, except the pouch *d, n, o, i*, fig. 4. Examining the root of the *epiglottis* I found a
hole:

hole *i, p*, fig. 3. being the real orifice of the bag *d, i, v, n*, fig. 4.

As all this lies above the *rima glottidis i*, fig. 3. or *i, b*, fig. 4. I concluded, that the voice, having passed the *glottis*, entered this membranous bag, *d, n, o, i*, by which the neck swelled, and out of which the air was forced by the contraction of the *latissimi colli*. I had often observed this swelling in some living apes, but now found out the reason of it, and was persuaded of the incapacity of this animal to modulate his voice so as to articulate words.

I then considered this remarkable passage of GALEN's, de Ufu Part. Ed. Charter. tom. IV. lib. 7. cap. XI. p. 461. *Foramen in utraque lingulae (epiglottidis) parte unum effecit natura, et foramini ipsi parte internâ ventriculum supposuit non parvum. In quem quum aer vias nactus amplas in animal ingreditur, rursusque exit, nihil in ventrem depellitur*; and what he, p. 466. further observes, *fissuram potius, quam foramen esse*.

When I compared this with the organ in the Cynocephali, fig. 3, 4. I was at a loss how to explain GALEN; for I could by no means apply those *ventriculi*, by which he seemed to have understood large capacities, to the small holes *b, i, k*, fig. 4. above the *rima glottidis i, b*, which, though much larger in the Cynocephali than in

men, could not be applied to this very particular definition of no small bags, *ventriculum non parvum*.

In November 1758 I dissected another monkey, in which the membranous bag, *d, n, o*, was much larger, so as to occupy almost the whole fore part of the neck, under the *latissimi colli*.

In the *Apella* (the 29th species of LINNÆUS, Syst. Nat. ed. XII. p. 42. or *simia caudata imberbis, cauda subprehensili, corpore fusco, pedibus nigris*) was no such a bag at all, nor any opening at the root of the *epiglottis*, which was entirely similar to that of dogs. I cannot forbear to mention, that in this monkey the *meatus*, or the *processus peritonæi*, were closed as in men. This I dissected in the year 1768, when I was Professor in the University of Groningen.

Here I got the opportunity, the year following, of dissecting two *papiones* or *spbinges* of LINNÆUS, *simie semi caudatæ ore vibrissato unguibus acuminatis*, spec. 6. p. 35. a male and a female; in which the *epiglottis* was likewise perforated, the *os hyoides* as in the former, but the pouch very small in comparison of the apes, who were very large.

As these parts are so apparent in a great many monkeys, and likewise in the ape or *Pitbecos*, I was very much surprized EUSTACHIUS did not discover them, especially

as he had taken great pains to pursue the anatomical doctrine of GALEN, as appears in the XLth plate, where he has given several figures of this organ. I was no less surprized that ALBINUS and MARTINS did not find this bag; and I wondered likewise very much, how Mr. D'AUBENTON, who has had the greatest opportunity of any anatomist, could pass over so striking a construction of this organ. I do not mention RIOLANUS, FALLOPIUS, GORTER, SYLVIUS, BLASIUS, and some others, because they had fixed their attention upon quite different parts.

§. 2. AS GALEN not only dissected the *Cebi*, or the *Cynocephalus*, who are all of the tailed or *caudati* kind, but the *Pithecus* or ape without a tail; and as the celebrated Dr. TYSON had found the organ of voice so similar to that of men in his *Pigmy*, I endeavoured to get one from the East Indies. For this purpose I offered a good sum of money to my correspondents to have a well-preserved Orang Outang, because none were to be met with in any collection of Natural History in Holland.

I soon got a female one in the year 1770, by the kindness and generosity of Dr. HOFFMAN, physician at Batavia, formerly my pupil; and the year 1771 another, by favour of Mr. HOPE, Director of the East India Company of Amsterdam, and Representative of his Most Serene Highness the Prince of Orange in the same Company,
 7 who

who was so good as to order not only a female, but a male one for my use; but this last was unluckily lost, with the whole ship, betwixt Java and the Cape of Good Hope.

These and the succeeding years were very favourable to Naturalists; for Professor ALLAMAND got a male Orang for the Museum of the University of Leyden; Mr. VANDER MEULEN, a great admirer of natural history (who has one of the finest collections at Amsterdam) received one, and Mr. VOSMAER got two for the celebrated collection of the Prince of Orange, all females. In the year 1777, Dr. VAN HOEY (a physician of great celebrity at the Hague, who has a very rich collection of natural curiosities) got a male (but a very young) Orang. Upon the whole, I had an opportunity of seeing seven Orangs, besides the living one, which was sent to his Highness the Prince of Orange.

All these resembled perfectly in shape and colour that of Mr. EDWARDS, which is still preserved in the British Museum.

Seven of those, I had seen, had no nails upon the great toes of the feet: it surprized me, therefore, to see them so distinctly represented by Professor ALLAMAND. I took the liberty to inform him of it; he corrected his description accordingly, p. 75. *ib. in fine*, which was easily

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done,

done, as the ſheets were not worked off at the preſs. I wrote likewiſe to Dr. KOOYſTRA, phyſician to the London Infirmary, to inquire about the Orang in the Britiſh Muſeum. The late Dr. MATY examined, at my requeſt, the Orang with him; and both declared, that not a ſingle mark of a nail was to be found upon the large toes of that ſpecimen, though Mr. EDWARDS had repreſented them on his 213th table ſo very large. Theſe two inſtances ſhew us, how little we can depend upon figures, if not drawn with great exactneſs.

The want of theſe nails, and of the ſecond phalanx of the large toes, is beyond any doubt a very remarkable character in this animal. Nature, however, ſeems to be inconstant ſometimes; for, upon the great toe of the right foot of the Orang in Dr. VAN HOEY's collection, there was a little nail and two phalanges. The ſingular red, long hair, and the ſhortneſs of the neck, form another very peculiar property; for in the living, as well as in all the reſt, I have obſerved the ſhoulders to riſe up to the ears; the lower and upper jaws were much projected forwards, as I ſhall ſhew in the anatomical deſcription of the Orang. The country they all came from was Borneo, from which iſland they are firſt ſent over to Java, and ſo to Holland by the Cape of Good Hope.

The

The Orang Outangs described by TULPIUS and TYSON came from Angola, and had both black hair, and large nails upon the great toes. I own the figures of these great men are very deficient in many respects: but, upon the whole, the animals are represented and described as very strong and muscular; whereas all the Orangs from Borneo, I have seen, were the contrary, and had long and very lean arms and legs.

To conclude, it seems very probable, that Africa furnishes a peculiar sort of apes which are not the *Pithecus* of the ancients, though these are not uncommon in Angola.

The organs of voice of the Angolese Orang, dissected by TYSON, are very different from those of the *Pithecus* which I dissected 1777. This one had the *os hyoides* like all the *papiones* or *sphinges*, &c.; the *epiglottis* perforated as in fig. 3. and 4. and therefore different from GALEN's description, and from TYSON's, who makes no mention at all of the one, nor of the two bags which GALEN describes, and which I found in the real Orang of Borneo; not only in one specimen, but in five, which I have dissected for that purpose.

To return to GALEN; I am very apt to think that he dissected an Asiatic Orang, from whom he took his description of the ventricles *a latere lingulae*, at the sides of the *epiglottis*; at least that he dissected such an organ, for

the bones of the *carpus* do not entirely agree with his description, though he seems to have been very exact and nice in his dissections. And indeed I wondered as often as I compared the structure of the *carpus* and *tarsus* of apes, monkeys, and dogs, with GALEN's *osteological* performances upon this subject: for though he describes but eight bones in the *carpus*, he mentions the ninth, which I have met with in all monkeys, apes, and dogs, and likewise in the Orang. The tenth is not easily seen, it being very much attached to the *os naviculare*. These bones I shall give the explanation of in the anatomical description of the Orang.

In the Angolese Orang, Dr. TYSON met with the vermicular process of the *intest. cæcum*, which I found very considerable in the Asiatic; but of which GALEN appears not to have had the least notion. Mr. D'AUBENTON has given the description and figure of the same little gut in the Gibbon, a species approaching to that of the Orang, and likewise an inhabitant of Asia, but also unknown to GALEN.

§ 3. I shall now proceed to the organ of speech itself, and describe it as it appeared in the first Orang I dissected in October 1770. And for the clearer understanding I shall add some figures to it; first, of the fore-part;
I
secondly,

secondly, of the *larynx* from the inside of the *pharynx*; and, lastly, of the inside of the *larynx* itself.

Fig. 2. N, O, P, represents the *os hyoides*; N, O, the basis; P the left *cornu*; N, O, the little graniform bones.

Q, T, U, the *thyroid* cartilage; V the *aspera arteria*.

Z, X the right ventricle cut open; R, S the left.

Y the hole leading into the bag. The ventricles form here a kind *meatus*, passing over the brim of the *thyroid* cartilage, under the *os hyoides*, towards the inside, where they form the fissures *a*, *b*, and *a*, *i*, fig. 5. I dissected this in the Anatomical Theatre of the University at Groningen in public.

In a second, which Mr. VOSMAER, the keeper of the Prince of Orange's cabinet, was so kind as to grant me for dissection, I found both these ventricles the same in every respect as the others, except that these last were of equal size.

In the third, which I dissected at the house of Dr. VAN HOEY at the Hague, the 31st of August, 1777, the two ventricles were smaller but of equal size on both sides. The animal was very young. The doctor preserves this preparation in his museum.

In a fourth, which I preserve intire in my collection, but whose organ of voice I examined the 30th of November

vember laſt, I found both the ventricles united ſo as to form but one.

The 6th figure gives a ſketch of it; *a, c, d, e, f, g, b*, *b*, is the ventricle, having, nevertheless, the two *mcatus's* *a* and *b*, and ſhewing evidently a kind of diviſion in *i*; *g, b*, making a ſmaller bag.

This bag deſcended downwards to the middle of the breaſt bone, and ſpread itſelf ſidewards over the *ſternomaſtoideus*, with appendices underneath the *cucullares*.

The *latiſſimi colli* adhered very much to the fore-part, but ſidewards; and under, from the muſcles of the neck, they were eaſily ſeparated by tearing gently, either with the top of the finger, or with the flat part of the handle of a diſſecting knife.

As this Orang was much larger than the former ones, and conſequently older, I dare not venture to determine, whether theſe ventricles or bags, which touch each other in the middle, grow together, ſo as to make but one bladder; or whether this may be a variety: becauſe in the Orang which was alive at the Hague, and the hiſtory of which I ſhall give by and by, there was likewise but one bag ſtill larger than theſe, and proceeding far over the *clavicles*, backwards under the *cucullares*, and before down two-thirds of the breaſt bone.

This accidental union can probably make no essential variety; for as these receptacles of air do not seem to serve for any modulation of voice, they will answer their proper purpose, whether united into one, or divided into two cavities. We very often see the kidneys united at the lower ends across the spine in men, without its occasioning any disturbance in the secretion or animal economy.

§ 4. I must now give the history of the so much celebrated Orang which belonged to the Prince of Orange, and died in January 1777. This was a female; when alive the head was always deep in the shoulders, and the animal seldom lifted it very high up. The man who took care of her observed a great quantity of air under the skin of the neck on both sides, which (being ignorant of these ventricles) he took for a dangerous disorder, and the symptoms of approaching death. I felt the neck myself in December 1776, and discovered the bags to be much larger than any I had dissected. I could remove the air easily with my hand from one side to the other, and divide it, as it were, into two parts. The bags appeared sometimes very turgid, sometimes collapsed.

She died not long afterwards, and was soon cut to pieces by the order of Mr. VOSMAER, to be stuffed for the Museum of his Serene Highness the Prince of Orange; but, as this cannot be done without preserving the face, a part of the skull, hands, and feet, it is very evident, that Mr. VOSMAER was

was obliged to cut off the head and the other extremities, and to destroy the most interesting parts for natural knowledge.

I was very sorry to hear the fate of this curious and uncommon creature, more especially as I had great reason to flatter myself with the dissection of the entire animal as soon as it was dead.

I need not remind any one of the particulars mentioned by Mr. FORSTER in the 2d volume of a Voyage round the World, p. 553.; nor of his rather too severe criticism upon the conduct of Mr. VOSMAER, the inspector of the Museum belonging to the hereditary Stadholder of the United Provinces. Mr. VOSMAER had, without doubt, no other intention but to preserve the fresh skin of this uncommon animal stuffed, for the cabinet of his benefactor, and not the least malevolent intention to prevent the dissection of the other parts not necessary for this purpose: for, when, by a special order of his Most Serene Highness the Prince of Orange, the remaining trunk was sent to me, I found the organ of voice not in the least hurt, and quite entire, as it is still to my great satisfaction. After having duly examined, dissected, and delineated the *viscera* of the breast and belly, I have put it in melasses, in a fine phial, in order to preserve so valuable a preparation, not only for my museum, but for natural knowledge in general.

There

There was no difference betwixt this organ and that delineated in the 6th figure, but in extent. The united ventricles covered the greatest part of the breast bone, and had several appendices, which insinuated themselves into all the interstices of the muscles of the neck and shoulders.

It had likewise two distinct *meatus's* coming from the inside of the organ at the sides of the *epiglottis*, as in fig. 5. and passing between the *os hyoides* and *thyroid* cartilage.

A large and vermicular process was attached to the *cæcum*; but the intestines were very different on the inside from those of men. The *os femoris* was kept in its socket only by a strong capsular ligament, there being no *ligamentum teres*: I had not observed one in the Orang which I preserve, and whose feet I dissected, to compare them with GALEN and others.

§ 5. Having given the structure of the organ of voice in five different Orangs, and demonstrated their conformity in every other respect but the union I mentioned in some, I shall proceed now to the internal part of the organ, as it is described by GALEN.

Fig. 5. shews the inside of the organ, which is represented in fig. 2. consequently of the same Orang.

d, e, f, is the *epiglottis* or *lingula*.

VOL. LXIX. X *g, h,*

g, b, k, l, the *cricoid* cartilage, divided in the middle, and expanded sidewards.

b, d, b, and *g, i, f*, the *arytenoid* cartilages.

i, a, and *a, b*, the holes or fissures at each side of the *epiglottis*; *b* and *i*, the cords which form the *rima glottidis*. All this answers exactly to the description given by GALEN.

The air which is forced by expiration out of the lungs, and passes with an accelerated velocity the *rima glottidis*, *b, i*, being stopped by the hollow *epiglottis* and the roof of the mouth, narrow nostrils, &c. rushes into these ventricles *z, x* and *q, r, s* fig. 2. or into the united large ventricle *a, b, d, e, f, g*, fig. 6. These are, as GALEN rightly observes, seemingly within the animal; for they are covered with the external integuments and the *latissimi colli*. From thence, or out of these ventricles, the air gets out again by the same fissures *a, i, a, b*, fig. 5. through the mouth and nostrils, without entering into the lungs again, or, as he expresses it, without entering into the belly of the animal, *rursusque exit, nihil in ventrem depellitur*; by *venter* is to be understood the inside of the body.

If this organ does not answer entirely to the description of GALEN, I do not know how to explain the quotation; for there is no animal, as yet known, whose organ of

of speech is more applicable to it, at least none of the monkey kind, as I observed before.

It is hardly to be conceived, how Dr. TYSON should have overlooked all this, and have pronounced the organ of voice of his pigmy to be exactly like that of men, as he has done p. 51.; and yet, it is not impossible, when we consider that he has overlooked other and more striking differences in his essay.

Nor is it probable that GALEN should have overlooked the large vermicular process of the *cæcum* and other things, if he had dissected the same kind of African Orang as TYSON did, unless he dissected the Organ of voice in the one, neglecting the intestines, and again the bones of the feet in another, as is often the case with anatomists, as I know by my own experience. This, however, seems probable, that GALEN dissected more than one species of *pithecos* or apes without a tail, and that even that species was different from the Angolese pigmy and from the Orang of Borneo.

§ 6. Having dissected the whole organ of voice in the Orang, in apes, and several monkeys, I have a right to conclude, that Orangs and apes are not made to modulate the voice like men: for the air passing by the *rima glottidis* is immediately lost in the ventricles or ventricle of the neck, as in apes and monkeys, and must conse-

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quently

quently return from thence without any force and melody within the throat and mouth of these creatures: and this seems to me the most evident proof of the incapacity of Orangs, apes, and monkies, to utter any modulated voice, as indeed they never have been observed to do.

I have already mentioned in the anatomical description of the Rein-Deer (p. 55. of Mr. ALLAMAND's addition to the XVth volume of the Count DE BUFFON) the surprising analogy of its organ of voice with that of the *Pitbeci* and *Cercopitbeci*. That of the Orang seems to have some analogy with that of frogs, whose voice, however, can be better modulated by their tongues. I have given a description of them in the Memoirs of the Society of Rotterdam. As I am afraid of having dwelt already too long on this subject, I shall here finish this essay; but promise to send the Society an account of the very extraordinary organ of voice of the *alouate* or *bourleur de Cayenne*, the *Simia Capucina* of LINNÆUS, n. 30. p. 42. the organ and *os hyoides* of which, &c. I dissected some time ago with all possible care and attention.

I am, &c.

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EXPLANATION OF THE FIGURES.

Fig. 1. represents the *pharynx* of the Orang Outang, from which the organs of voice, fig. 2. and 5. are delineated.

A, D, B, C, the tongue from behind.

D, B, C, M, the *palatum molle*, on the back part of which the *uvula* B, L is seen.

B, E, G, H, F, the *pharynx*, divided lengthways in the middle from E to G.

K, M, J, the passage from the mouth into the *œsophagus* S, K, G, H. Within this is seen the *epiglottis* and the *glottis* shut by the *arytenoid* cartilages.

Fig. 2. is the same organ of voice from the fore-part.

N, O, P, the *os hyoides*; N and O the little graniform bones; P, the left *cornu*.

Q, T, U, the *thyroid* cartilage; V the *aspera arteria*.

R, S, the left ventricle intire.

W the right, cut open to see the orifice of the duct X from the bag.

Fig. 3. The back part of the tongue and the *glottis* of a monkey.

a, b,

a, b, the *epiglottis*; *a, b, r, g*, the root and back part of the tongue; *c, s, t, d*, the *œsophagus* laid open.

u, the *aspera arteria*.

f, e, the *capitella* of the *arytenoid* cartilages.

e, the upper part or top of the little cartilage between the *arytenoid* cartilage and the *epiglottis*, which I have likewise met with in men, but less prominent.

i, f, the *rima glottidis*.

p, i, the hole at the root of the *epiglottis*.

Fig. 4. the inside of the *larynx* in profil.

a, b, c, d, the *epiglottis*; *e*, the cartilage mentioned in fig. 3.

f, b, the *arytenoid* cartilage; *f, g*, the *capitellums* forming a kind of crooked hook.

i, m, the cord of the *glottis*.

i, k, b, the lateral *sinus* above the *rima glottidis*, forming a pretty large ventricle in these animals.

i, m, l, the *cricoid* cartilage.

d, n, o, the ventricle, into which the air, coming through the hole at the root of the *epiglottis*, enters.

Fig. 5. the same *larynx*, represented in fig. 1. opened, to see the inward parts.

a, the union of the cords forming the *rima glottidis*.

I

a, b,

a, b, a, i, the holes or orifices by which the air enters into the two ventricles *r, s* and *z, x*, fig. 2.

b, c, b, d, the right *arytenoid* cartilage, with its *capitulum d*.

i, g, f, the left *arytenoid* cartilage.

f, e, d, the *epiglottis*.

g, b, k, l, the *cricoid* cartilage, divided and dilated.

k, m, n, l, the wind-pipe.

Fig. 6. the fore part of the Orang, which I preserve entire in my museum. The skin of the neck and the *latissimi colli* are laid open, to shew the ventricles, &c.

A, B, C, the lower jaw-bone; *A, D, A, E*, the *genio hyoides*.

F, G, the *cornua* of the *os hyoides*.

H, I, the *thyroid* cartilage; *K*, the *cricoid*.

L, M, the sub-maxillary glands.

a, c, d, e, f, g, b, b, the large bladder formed by the union of the two ventricles, of which *i, f*, is a mark.

a and *b*, the two *meatus's* entering towards the inside of the *larynx*, betwixt the *thyroid* cartilage and the *os hyoides*.



XV. *Account of the Effects of Lightning on Board the Atlas. By Allen Cooper, Esq. Master of the Atlas East India-man. In a Letter to Joseph Banks, Esq. P. R. S.*

Read January 19, 1779.

DEAR SIR,

Mother-Bank,
Jan. 4, 1779.

AS I am desirous of acquainting you with every observation made either by my friend Dr. LIND or myself during the voyage, I cannot help troubling you with an account of the melancholy effects of the lightning on board my ship the Atlas on the 31st of Dec. 1778. The morning and forenoon of that day were clear and cold, with a strong dry wind from the N.W. which by Dr. LIND's wind-gage generally sustained a column of water of two inches^(a), and was so high as to prevent boats coming off to the ship. At 3 P.M. a squall from the N.N.W. came with a violence scarcely credible, attended with very heavy rain, large hail, and the most severe lightning, which struck our main-mast-head, descended down the mast or its rigging, and entered the gun-deck somewhere nigh the main hatchway. Those

(a) A force equal to lb. 10,4.

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who

who were employed in letting down the sheet cable (as we had then brought both our anchors a-head) received very smart shocks, and were witnesses to the fire going out at several parts of the ship, and to an explosion equal to that of a well-charged cannon, accompanied with a most sulphureous smell which lasted all that day and night. It was not till the squall abated that our attention was called to the masts, when we saw one of our best seamen hanging by his feet in the main catharpins struck dead; another in the main-top was so miserably scorched as to remain senseless, and now continues in a dangerous way. The boatswain's mate, who was nigh him, had his arm so much hurt by the shock he received, as not to recover the use of it for half an hour. All possible means were used to save the poor fellow who had received the fatal stroke; but there appeared no signs of returning life in him. His face was quite livid; and from the livid colour of the scorched places it appeared the lightning had entered his head, come out again on the left side of his neck, and spread itself down his left side and over his legs. The other man was struck down in the main-top, his back much scorched, and on the inside of his right leg the stocking burst open a little below the knee. I have sent the cloaths of the man who suffered, as also the stocking of the other man. Our top-gallant-masts, at

the time they were struck, had no iron work upon them^(b). Upon a careful examination, no visible track of the lightning could be found upon the masts, or any part over which it had passed, nor was any damage done to the ship, masts, or rigging.

(b) On account of the season of the year, in which lightning is so extremely unusual, it had not been thought necessary to fix the conductor.

XVI. *Extracts of Three Letters from John Longfield, M.D. at Corke in Ireland, to the Astronomer Royal, containing some Astronomical Observations; together with the Longitude of Corke, deduced from the said Observations, by the Astronomer Royal.*

Read February 11, 1779.

S I R,

Corke,
Oct. 1, 1773.

ENCOURAGED by Mr. WALSH, with whom I have the pleasure of being acquainted, and who has favoured me with a letter to you; and being confident of your inclination to promote Geography and Astronomy; I trouble you with a few observations, and request your advice and assistance in the prosecution of my favourite study.

Last year I built a very solid and commodious, though small observatory, close to my house. The top of one of the hills which surround this city would have been a more eligible situation; but as my profession confines me to the town I had no choice. To make up its deficiency in height, I have adapted the upper part of my house to the

Y 2

same

same purpose, which serves very well for instruments that do not require great solidity: From hence I have a very extensive view all round. In the observatory is a solid pier, sunk deep in the ground, on the top of which stands the transit instrument. It is of tin, three feet and a half long, and made by Mr. MONK. The other instruments I have are the following:

An equal altitude instrument.

A quadrant of one foot radius, by Mr. BIRD.

Another of two feet and a half radius, of the French construction, not a bad one with regard to the divisions, but inconvenient.

An excellent telescope, with a treble object-glass, by Mr. DOLLOND. It magnifies about 140 times, and is of the latest improvement.

A parallactic instrument, with a common telescope five feet long, and a *reticule rhomboide*.

Two astronomical clocks, with wooden pendulums, one of which goes full as well as Mr. WOOLLASTON'S. The alterations in their going seem to be owing to moisture. I constantly compare them with SMEATON'S hygrometer.

With the assistance of a particular friend, who is an excellent mathematician and a very good observer [Mr. ELIAS MAINADUC], I have taken a great number of
meridian

meridian altitudes of the Sun, and of stars to the North and South of the zenith. The mean of both quadrants makes the latitude $51^{\circ} 53' 54''$.

App. time.

1772, July	4	at	12	52	0	α 's 2d fat. imm. hazy.
	18	-	12	41	23	1st fat. imm.
Aug.	3	-	10	58	27	ditto, ditto.
Sept.	20	-	8	21	16	ditto, emerf.
Oct.	6	-	6	46	12	ditto, ditto, hazy.

They were all observed with the greatest magnifying power.

The weather has been almost continually cloudy for these six months past, which has prevented me from making any observation of consequence except on April, 29th, an occultation of α Cancr. by the Moon at 11h 23' 25" apparent time, and an immersion of Jupiter's first fatellite at 12h 52' 29" on August the 22d.

I should be much obliged to you for the corresponding observations to the above, if it would not be too much trouble.

I am, &c.

S I R,

S I R,

Corke,
Feb. 18, 1774.

I HAVE now had a full trial of SMEATON's hygrometer, and think it a very useful and agreeable instrument. It is capable of a considerable degree of exactness, but not sufficient for any thing which requires great accuracy. It absorbs the moisture of the air much more readily than it parts with it; and I have great reason to believe, that, as the gravity of the air differs, it will point to different degrees on the scale, the degree of moisture or dryness being the same. If the cord is too much twisted it will require frequent adjustments, and I know that the extreme degree of moisture is very uncertain (though that of dryness is not so) and different persons will adjust it very differently at that point.

My telescope I have found, by theory and experiment, to magnify 134 times.

My friend, whom I mentioned to you, has been making observations for these thirty years. He has a good clock; with a common pendulum; a quadrant of two feet radius; a five feet telescope of two glasses, with a common micrometer; an excellent reflector of SHORT'S, of eighteen inches focus; and is getting a transit instrument. His observations

observations gave him the latitude of Corke $51^{\circ} 54'$, and the longitude $34\frac{1}{2}$ minutes of time West of Greenwich.

I am, &c.

1773	Clock differs from mean Time.	Gains or loses <i>per diem</i> .	Mean of Therm. in obs.	Mean of Hygr.	Observations.
Sept. 11	-0 17.0		60	54	{ Hygr. was put into the observ. clock was losing 0.5 <i>per diem</i> .
12	-0 17.0	-0.0	60	50	
13	-0 17.5	-0.5	60	38	
14			60	36	
15	-0 18.5	-0.5	60	30	
16			60	26	
17			60	20	
18	-0 20.0	-0.5	60	16	
19			60	14	
20			60	18	
21			60	14	
22	-0 21.0	-0.25	55	14	
23			53	20	
24			60	35	
25			60	42	
26			60	35	
27			58	25	
28			58	33	
29	-0 27.0	-0.8	54	32	
30			55	32	
Oct. 1	-0 28.7	-0.8	55	40	
2			53	28	

1773	Clock dif- fers from mean time.	Gains or loses <i>per</i> <i>diem.</i>	Mean of Therm. in obs.	Mean of Hygr.	Observations.
Oct. 3	" "		48	30	{ Hygrometer adjusted, much lengthened.
4			48	28	
5			50	20	
6	-0 33.0	-0.8	53	22	
7			50	24	
8	-0 35.0	-1.0	55	25	
9			54	22	
10	-0 37.0	-1.0	54	20	
11			53	25	
12	-0 39.0	-1.0	53	14	
13			50	15	
14			54	18	
15			50	20	
16			53	16	
17			54	18	
18			54	18	
19	-0 49.5	-1.5	54	20	
20			53	30	
21			54	42	
22			54	38	
23			53	30	
24	-0 56.5	-1.4	54	25	Hygrometer adjusted.
25			53	24	
26			54	20	
27			56	18	
28			54	20	
29			45	24	

1773	Clock differs from mean time.	Gains or loses per diem.	Mean of Therm. in obs.	Mean of Hygr.	Observations.
Oct. 30	-1 2.5	-1.0	45	30	
31			45	36	
Nov. 1			46	40	
2			45	30	
3			45	32	
4			44	38	
5			45	35	
6			44	40	
7	-0 59.0	+0.4	48	48	
8			46	49	
9			46	59	
10			44	44	
11	-0 58.5	+0.1	44	38	{ Hygr. adjusted, had lengthened 1 st only.
12			43	34	
13	-0 58.0	+0.2	46	37	
14			45	40	
15	-0 58.0	+0.0	46	34	
16			44	39	
17	-0 57.0	+0.5	42	36	
18			40	30	
19			41	28	
20	-0 55.5	+0.5	41	28	
21			45	30	
22	-0 54.5	+0.5	36	31	
23			35	32	
24	-0 52.7	+0.9	38	29	

1773	Clock dif- fers from mean time.	Gains or loses <i>per</i> <i>diem.</i>	Mean of Therm in obs.	Mean of Hygr.	Observations.
Nov. 25	" "		36	28	
26	-0 51.0	+0.9	40	29	
27			43	48	
28			46	40	
29			40	33	
30	-0 49.0	+0.5	43	32	
Dec. 1	-0 48.5	+0.25	45	34	
2			47	36	
3			46	37	
4	-0 48.0	+0.1	42	32	
5	-0 47.5	+0.5	38	30	
6			40	32	
7			38	29	
8	-0 44.5	+1.0	38	29	
9			38	30	
10			45	48	
11			38	43	
12			44	45	
13	-0 39.5	+1.0	46	30	
14			48	37	
15			47	44	
16			47	42	
17			47	42	
18			47	42	
19			46		
20	-0 31.5	+1.1	48		

1773	Clock dif- fers from mean time.	Gains or loses <i>per</i> <i>diem.</i>	Mean of Therm. in obs.	Mean of Hygr.	Observations.
Dec. 21	" "		50		
22			42		
23	-0 27.5	+1.3	39		
24	-0 26.0	+1.5	36		
25	-0 24.5	+1.5	34		
26			37		
27			40		
28	-0 18.5	+2.0	36		
29			37		
30	-0 15.0	+1.7	34		
31	-0 13.0	+2.0	36		
1774 Jan. 1			36		
2	-0 8.4	+2.3	32		
3			30		
4	-0 3.8	+2.3	36		
5	-0 2.0	+1.8	42		
6			43		
7			40		
8			46		
9			42		
10			41		
11			40		
12			44		
13			45		
14	+0 7.0	+1.0	43		
15			46		

1774	Clock differs from mean time.	Gains or loses per diem.	Mean of Therm. in ob.	Mean of Hygr.	Observations.
Jan. 16	+0 9.0	+1.0	48		
17			45		
18			32		
19			39	39	
20			43	45	
21			43	45	
22			43	49	
23	+0 18.5	+1.3	38	37	
24			35	30	
25			35	36	
26			42	33	
27	+0 24.5	+1.5	38	44	
28			41	34	
29	+0 26.5	+1.0	37	32	
30			34	32	
31	+0 28.5	+1.0	35	24	
Feb. 1			32	28	
2	+0 30.0	+0.7	32	24	
3			32	24	
4	+0 31.0	+0.5	33	25	
5			34	26	
6	+0 32.0	+0.5	42	26	
7			42	30	
8	+0 32.5	+0.25	37	26	
9			39	27	
10			44	47	

1774	Clock differs from mean time.	Gains or loses per diem.	Mean of Therm. in obs.	Mean of hydr.	Observations.
Feb. 11	" "		42	40	
12			49	50	
14	+0 33.5	+0 5	46	49	
15			41	41	
16			40	37	
17	+0 35.0	+0.5	41	30	
18	+0 35.5	+0.5	38		

My clock was bought three years since at an auction, among the collection of clocks of some gentleman, who was curious in that way in London. As soon as it arrived I bought it from the purchaser, a watch-maker: the name is HUGHES. It beats dead seconds, goes a month, and is finished both inside and out with so much pains and elegance, that the clock-makers inform me, it must have cost forty pound. It had a common pendulum with a heavy bob. I got a wooden pendulum and a new crutch applied to it exactly according to Mr. LUDLAM's directions, except that the spring is longer. For four months after it was put up it went irregularly, upon which I fastened it to the wall with screw bolts and large washers. Since that time, May 1772, it never lost more than 2" *per diem*, or gained more than 2".5, nor did it ever differ more than 1" in its daily rate on any two successive days.

S I R,

Corke,
Feb. 17, 1779.

I SEND you all the observations of any consequence that have been made here. Some of them are by Mr. NEWENHAM, a young gentleman of considerable abilities. He lives on a hill, about 2400 yards E. [answering to a difference of meridians of $7\frac{2}{3}$ seconds of time] and 600 N. of my observatory [answering to 18" difference of latitude] and has a clock with a wooden pendulum; a transit telescope of 30 inches in length, with an achromatic object-glass; and a reflecting telescope of eighteen inches focus, made by DOLLOND, magnifying 70 times very distinctly. Mr. MAINADUC's observatory is 1600 feet due W. of mine [answering to a difference of meridian of five seconds of time]. The eclipse of the Sun was observed there, for the convenience of the side-glass, with a transit telescope of small magnifying power, placed on rack-work; so that, though it was a very good observation, the beginning may be reckoned three or four seconds sooner. I was unluckily interrupted at the instant. The end was not seen. The longitude of Corke is, I believe, sufficiently determined by the occultations. I have calculated some of them. If you should
take

take that trouble; I shall be much obliged to you if you will let me know the result.

The account I sent you of the going of one of my clocks, I am afraid, is not worth laying before the Royal Society, as no conclusion can be deduced from it; but I am certain, that moist weather, for any length of time, makes it go slower, probably by increasing the weight of the rod: perhaps covering it with tin-foil would prevent its imbibing moisture. The other clock, with a mahogany pendulum, does not go well, as the fibres of the wood are not strait, and it warps from the changes of the weather.

I have taken some pains to fix the hygrometer to some standard, but in vain. One I have had about five years, though adjusted last summer, has almost lost the power of absorbing moisture; so that its contracting is to its lengthening as 1 to 3.

In 1774 I sent my achromatic telescope to DOLLOND and got another from him much better in every respect magnifying 150 times, with an achromatic object-glass micrometer, and a very firm stand and polar axis. It has but one set of sliding tubes at the object end, yet it is very steady, and answers perfectly well, when once it is fixed to the object.

Being

Being at one time intent on making some observations with the cross wires in your manner, I got a stand made with an arm, to carry a lantern that should follow the motion of the telescope, and applied a solid illuminator to it occasionally. The account of this I first met with in the preface to your admirable observations.

I changed the object of my transit telescope for an achromatic one, got a solid illuminator to it, and put in very fine wires, by all which it is much improved.

I am, &c.

Eclipses

Eclipses of Jupiter's first satellite.

	App. time.	
1772, July 18	12 41 23	{ Immersion, SHORT'S 18 inch R. M. P. 130, by Mr. MAINADEC.
Aug. 3	10 58 27.5	Immersion, achrom. 134.
Sept. 20	8 21 16	Emersion, ditto.
Oct. 6	6 46 12	Emersion, ditto.
1773, Aug. 22	12 52 29	Immersion, ditto.
Nov. 24	10 40 16.5	Emersion, ditto.
1774, Nov. 13	10 20 27	Emersion, achrom. 150.
29	8 34 19	Emersion, ditto.
1775, Jan. 14	8 43 23	Emersion, ditto.
Mar. 1	9 13 16	Emersion, ditto.

By Mr. GEORGE NEWENHAM.

1774, Nov. 29	8 34 42	Emersion, refl. DOLLOND, 70 M. P.
Dec. 8	4 54 56	Emersion, ditto.
22	6 38 45	Emersion, ditto.
1775, Jan. 14	8 43 34	Emersion, ditto.
Sept. 29	16 47 31	Immersion, ditto.
Oct. 1	11 16 41.5	Immersion, ditto.
22	17 1 53.5	Immersion, ditto.
24	11 31 25	Immersion, ditto.

Occultations.

	App. time.	
1773, Apr. 29	11 23 25	1 α Cancr., immersion.
1774, Apr. 14	6 41 49	α Tauri, emerfion.
1775, Sept. 14	11 7 1	γ Tauri, immersion.
	20 13 12	α Tauri, emerfion, uncertain to two seconds.
1777, Sept. 21	10 26 39	1 δ Tauri, immersion.
	10 43 28	2 δ Tauri, immersion.

By Mr. NEWENHAM.

1774, Nov. 18	14 35 42	α Tauri, immersion.
	15 52 27	α Tauri, emerfion.
1776, Jan. 8	17 59 17.5	α Leonis, immersion.
	18 48 55.5	α Leonis, emerfion.

Eclipse of the Sun.

1778, June 24 | 2 57 16 | Beginning, Mr. MAINADUC.

Magnetic variation 24° W. in July 1778.

The Longitude of Corke settled from the foregoing observations compared with others made at the Royal Observatory at Greenwich. -- By Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.

THE observations made at the Royal Observatory at Greenwich nearest to those made at Corke are as follows, and the error of the Nautical Almanac with respect to the time observed is set down, and also the correction of the Nautical Almanac, with respect to the time observed, and reduced to the effect of a $3\frac{1}{2}$ feet telescope, which shews the immerfions of the first fatellite sooner, and the emerfions later, than the fix feet reflector does by about $13''$.

		Apparent time.	Telescope.	State of air.	Corr. of Nautical Almanac.	Correction of Naut. Almanac for $3\frac{1}{2}$ telescope.
		h ' "	Feet.		' "	' "
1772, July 11	Im.	11 22 25	$3\frac{1}{2}$	- - -	-0 11	-0 11
Aug. 26	Em.	14 4 22	6	Air clear.	-0 19	-0 6
Sept. 27	Em.	10 52 43	$3\frac{1}{2}$	Air very clear.	-0 12	-0 12
Oct. 13	Em.	9 17 4	$3\frac{1}{2}$	Air clear.	-0 22	-0 22
1773, Aug. 31	Im.	9 51 57	6	Air clear.	-0 3	-0 16
1774, Sept. 10	Im.	15 28 31	6	{ Air clear & 4's belts distinct.	+0 57	+0 44
Dec. 29	Em.	11 3 48	6	- - -	-0 4	+0 9
1775, Feb. 22	Em.	7 49 37	6	Air clear.	+0 29	+0 42
Aug. 7	Im.	14 53 55	6	Air very clear.	+1 28	+1 15
Oct. 22	Im.	17 37 1	6	Air very clear.	+1 33	+1 20

A a 2

Hence

Hence the times of the Nautical Almanac corrected and reduced to the effect of a $3\frac{1}{2}$ feet telescope, and compared with the observations made at Corke, to find the difference of meridians of Greenwich and Corke, are as follows:

Hence the difference of meridians of Greenwich and Corke is $33' 57''$ of time, and the longitude of Corke is $8^{\circ} 29' 15''$ West of Greenwich. The latitude of Corke, as determined by Dr. LONGFIELD, by a mean from two quadrants is $51^{\circ} 53' 54''$ North.

By Mr. NEWENHAM'S observations compared in like manner the difference of meridians of Greenwich and his observatory is $34' 11''$, which, according to Dr. LONGFIELD'S observations, allowing $8''$ for the difference of meridians,

meridians, owing to the distance of the two observatories, should be $33' 49''$, which latter result is most to be depended upon. The latitude of Mr. NEWENHAM's observatory being $18''$ greater than that of Dr. LONGFIELD's, according to the measured distance, is $51^{\circ} 54' 12''$ North.

XVII. *The Latitude of Madrafs in the East Indies, deduced from Observations made by William Stephens, Chief Engineer. Communicated by John Call, Esq. F. R. S.*

Read February 11, 1778.

		Latitudes of Madrafs,	
Time.	What objects.	from Southern Z. dist.	from Northern Z. dist.
1776, Oct.	4. Sun	13 3 58	— —
	5. Sun	13 4 20	— —
	19. Sun	13 4 16	— —
	22. Sun	13 3 56	— —
Nov.	1. β Caffiopea	— —	13 5 42
	α Caffiopea	— —	13 5 58
	γ Caffiopea	— —	13 5 44
	δ Caffiopea	— —	13 5 39
	2. δ Capricorn	13 4 29	— —
	Fomalhaut	13 4 29	— —
	β Ceti	13 3 55	— —
	3. Moon, this rejected in medium,	— —	13 6 58
	10. Sun, very good,	13 4 9	— —
	γ Lacerta	— —	13 5 25
	λ Andromeda (called $\iota\lambda$)	— —	13 5 25
	β Caffiopea	— —	13 5 42
	α Caffiopea	— —	13 15 9
		13 4 11	13 5 37

Mean latitude $13^{\circ} 4' 54''$ North.

The above observations were taken with an astronomical brass quadrant on the top of the house usually inhabited by the chief engineer.



XVIII. *Account of an Infant Musician.* By Charles Burney, *Doctor of Music and F. R. S.*

Read February 18, 1779.

TO DR. WILLIAM HUNTER, F. R. S.

S I R,

St. Martin's-street,
Feb. 9, 1779,

AS your curiosity seemed much excited by the extraordinary accounts of the Norwich musical child, and as you expressed some desire to know in what particulars his performance was wonderful, and disposition to music superior to that of other children of the same age: after making all the inquiries my leisure and opportunities would allow, and repeatedly hearing and studying him, I have drawn up the following account, which, if it does not appear too trivial, I should be glad you would do me the honour of presenting to the Royal Society, as a mark of my respect and veneration for that learned Body, who, as their inquiries extend to all parts of Nature, will perhaps not disdain to receive a narrative of the uncommon exertions of the human faculties at a more early period of life than they usually develope.

I have the honour to be, &c.

THAT reason begins to dawn, and reflexion to operate, in some children much sooner than in others, must be known to every one who has had an opportunity of comparing the faculties of one child with those of another. It has, however, seldom been found, that the senses, by which intelligence is communicated to the mind, advance with even pace towards perfection. The eye and the ear, for instance, which seem to afford reason its principal supplies, mature at different periods, in proportion to exercise and experience; and not only arrive at different degrees of perfection during the stages of infancy, but have different limits at every period of human life. An eye or ear that only serves the common purposes of existence is intitled to no praise; and it is only by extraordinary proofs of quickness and discrimination in the use of these senses, that an early tendency to the art of painting or music is discovered.

Many children, indeed, seem to recognize different forms, persons, sounds, and tones of voice, in very early infancy, who never afterwards endeavour to imitate forms by delineation, or sounds by vocal inflexions.

As drawing or design may be called a refinement of the sense of sight, and practical music of that of hearing; and as a perfection in these arts at every period of life,
from

from the difficulty of its attainment, and the delight it affords to the admirers and judges of both, is treated with respect, a premature disposition to either usually excites the same kind of wonder as a phenomenon or prodigy.

But as persons consummate in these arts, and who are acquainted with the usual difficulties which impede the rapid progress of common students, can only judge of the miraculous parts of a child's knowledge or performance, it will be necessary, before I speak of the talents peculiar to the child who is the subject of the present inquiry, to distinguish, as far as experience and observation shall enable me, between a common and supernatural disposition, during infancy, towards the art of music.

In general a child is not thought capable of profiting from the instructions of a music-master till five or six years old, though many have discovered an ear capable of being pleased with musical tones, and a voice that could imitate them, much sooner. The lullaby of a nurse during the first months of a child's existence has been found to subdue peevishness, and, perhaps, divert attention from pain; and in the second year it has often happened, that a child has not only been more diverted with one tune or series of sounds than another, but has had sufficient power over the organs of voice to imitate the inflexions by which it is formed; and these early proofs of

what is commonly called musical genius would doubtless be more frequently discovered if experiments were made, or the mothers or nurses were musically curious. However, spontaneous efforts at forming a tune, or producing harmony upon an instrument so early, have never come to my knowledge.

The arts being governed by laws built on such productions and effects as the most polished part of mankind have long agreed to call excellent, can make but small approaches towards perfection in a state of nature, however favourable may be the disposition of those who are supposed to be gifted with an uncommon tendency towards their cultivation. Nature never built a palace, painted a picture, or made a tune: these are all works of art. And with respect to architecture and music, there are no models in nature which can encourage imitation: and though there is a wild kind of music among savages, where passion vents itself in lengthened tones different from those of speech, yet these rude effusions can afford no pleasure to a cultivated ear, nor would be honoured in Europe with any better title than the howlings of animals of an inferior order to mankind.

All therefore that is really admirable in early attempts at music is the power of imitation; for elegant melody and good harmony can only be such as far as they correspond

respond with or surpass their models: and as melody consists in the happy arrangement of single sounds, and harmony in the artificial combination and simultaneous use of them, an untaught musician becomes the inventor of both; and those who are at all acquainted with the infancy of such melody and harmony as constitute modern music, can alone form an idea of the rude state of both when an individual discovers them by the slow process of experiment.

Every art when first discovered seems to resemble a rough and shapeless mass of marble just hewn out of a quarry, which requires the united and successive endeavours of many labourers to form and polish. The zeal and activity of a single workman can do but little towards its completion; and in music the undirected efforts of an infant must be still more circumscribed: for, without the aid of reason and perseverance he can only depend on memory and a premature delicacy and acuteness of ear for his guides; and in these particulars the child of whom I am going to speak is truly wonderful.

WILLIAM CROTCH was born at Norwich July 5, 1775. His father, by trade a carpenter, having a passion for music, of which however he had no knowledge, undertook to build an organ, on which, as soon as it would speak, he learned to play two or three common tunes,

such as *God save great GEORGE our king*; *Let ambition fire thy mind*; and *The Easter Hymn*; with which, and such chords as were pleasing to his ear, he used to try the perfection of his instrument.

I have been favoured with several particulars concerning his son's first attention to music from ROBERT PARTRIDGE, esquire, a gentleman of rank in the Corporation of Norwich, who, at my request, has been so obliging as to ascertain many curious facts, the truth of which, had they rested merely on the authority of the child's father or mother, might have been suspected; and transactions out of the common course of nature cannot be too scrupulously or minutely proved.

My correspondent, of whose intelligence and veracity I have the highest opinion, tells me, that I may rest assured of the authenticity of such circumstances as he relates from the information of the child's father, who is an ingenious mechanic, of good reputation, whom he knows very well, and frequently employs, as these circumstances are confirmed by the testimony of many who were witnesses of the child's early performance; and he adds, that he has himself seen and heard most of the very extraordinary efforts of his genius.

About Christmas 1776, when the child was only a year and a half old, he discovered a great inclination for
music,

music, by leaving even his food to attend to it when the organ was playing: and about Midsummer 1777. he would touch the key-note of his particular favourite tunes, in order to persuade his father to play them. Soon after this, as he was unable to name these tunes, he would play the two or three first notes of them when he thought the key-note did not sufficiently explain which he wished to have played.

But, according to his mother, it seems to have been in consequence of his having heard the superior performance of Mrs. LULMAN, a musical lady, who came to try his father's organ, and who not only played on it, but sung to her own accompaniment, that he first attempted to play a tune himself: for, the same evening, after her departure, the child cried, and was so peevish that his mother was wholly unable to appease him. At length, passing through the dining-room, he screamed and struggled violently to go to the organ, in which, when he was indulged, he eagerly beat down the keys with his little fists, as other children usually do after finding themselves able to produce a noise, which pleases them more than the artificial performance of real melody or harmony by others.

The next day, however, being left, while his mother went out, in the dining-room with his brother, a youth
of

of about fourteen years old, he would not let him rest till he blew the bellows of the organ, while he sat on his knee and beat down the keys, at first promiscuously; but presently, with one hand, he played enough of *God save great GEORGE our King* to awaken the curiosity of his father, who being in a garret, which was his work-shop, hastened down stairs to inform himself who was playing this tune on the organ. When he found it was the child, he could hardly believe what he heard and saw. At this time he was exactly two years and three weeks old, as appears by a copy I have obtained of the register in the parish of St. George's Colgate, Norwich, signed by the reverend Mr. TAPPS, minister. Nor can the age of this child be supposed to exceed this account by those who have seen him, as he has not only all the appearance, but the manners, of an infant, and can no more be prevailed on to play by persuasion than a bird to sing.

It is easy to account for *God save great GEORGE our King* being the first tune he attempted to play, as it was not only that which his father often performed, but had been most frequently administered to him as a narcotic by his mother, during the first year of his life. It had likewise been more magnificently played than he was accustomed to hear by Mrs. LULMAN, the afternoon before he became a practical musician himself; and, previous

vious to this event, he used to teize his father to play this tune on his organ, and was very clamorous when he did not carry his point.

When his mother returned, the father, with a look which at once implied joy, wonder, and mystery, desired her to go up stairs with him, as he had something curious to shew her. She obeyed, imagining that some acquaintance or friend was arrived, or that some interesting event had happened during her absence; but was as much surprized as the father on hearing the child play the first part of *God save great GEORGE our King*. The next day he made himself master of the treble of the second part; and the day after he attempted the base, which he performed nearly correct in every particular, except the note immediately before the close, which, being an octave below the preceding sound, was out of the reach of his little hand.

In the beginning of November 1777 he played both the treble and base of *Let ambition fire thy mind*, an old tune which is, perhaps, now better known by the words to which it is sung in *Love in a Village*, *Hope, thou nurse of young desire*.

Upon the parents relating this extraordinary circumstance to some of their neighbours, they laughed at it; and, regarding it as the effect of partial fondness for their

their child, advised them by no means to mention it, as such a marvellous account would only expose them to ridicule. However, a few days after, Mr. CROTCH being ill, and unable to go out to work, Mr. PAUL, a master-weaver by whom he was employed, passing accidentally by the door, and hearing the organ, fancied he had been deceived, and that CROTCH had stayed at home in order to divert himself on his favourite instrument; fully prepossessed with this idea, he entered the house, and, suddenly opening the dining-room door, saw the child playing on the organ while his brother was blowing the bellows. Mr. PAUL thought the performance so extraordinary, that he immediately brought two or three of the neighbours to hear it, who propagating the news, a croud of near a hundred people came the next day to hear the young performer, and, on the following days, a still greater number flocked to the house from all quarters of the city; till, at length, the child's parents were forced to limit his exhibition to certain days and hours, in order to lessen his fatigue, and exempt themselves from the inconvenience of constant attendance on the curious multitude.

This account agrees in most particulars with a letter I received from Norwich, and of which the following is an extract.

“ There

“ There is now in this city a musical prodigy, which
“ engages the conversation and excites the wonder of
“ every body. A boy, son to a carpenter, of only two
“ years and three quarters old, from hearing his father
“ play upon an organ which he is making, has discovered
“ such musical powers as are scarcely credible. He
“ plays a variety of tunes, and has from memory repeated
“ fragments of several voluntaries which he
“ heard Mr. GARLAND, the organist, play at the cathedral.
“ He has likewise accompanied a person who
“ played upon the flute, not only with a treble, but has
“ formed a base of his own, which to common hearers
“ seemed harmonious. If any person plays false, it
“ throws him into a passion directly; and though his
“ little fingers can only reach a sixth, he often attempts
“ to play chords. He does not seem a remarkable
“ clever child in any other respect; but his whole
“ soul is absorbed in music^(a). Numbers crowd daily

(a) This opinion seems to have been too hastily formed; for, independent of his musical talent, he appears to me possessed of a general intelligence beyond his age: and he has discovered a genius and inclination for drawing, nearly as strong as for music; for whenever he is not at an instrument, he usually employs himself in sketching, with his left-hand, houses, churches, ships, or animals, in his rude and wild manner, with chalk on the floor, or upon whatever other plain surface he is allowed to scrawl. Painters may, perhaps, form some judgment of his music by his drawings.

“ to hear him, and the musical people are all amazed^(b) ment.”

The child being but two years and eight months old when this letter was written, his performance must have appeared considerably more wonderful than at present: for as he seems to have received scarce any instructions, and to have pursued no regular course of study or practice since that time, it can hardly be imagined that he is much improved. However, experience must have informed him what series or combination of sounds was most offensive to his ear; but such is his impetuosity that he never dwells long on any note or chord, and indeed his performance must originally have been as much under the guidance of the eye as the ear, for when his hand unfortunately falls upon wrong notes, the ear cannot judge till it is too late to correct the mistake. However, habit, and perhaps the delicacy and acuteness of another sense, that of feeling, now direct him to the keys which he presses down, as he hardly ever looks at them.

(b) His father, who has lately been in London, and with whom I have conversed since this account was drawn up, all the particulars of which he has confirmed, told me, that when he first carried the child to the cathedral he used to cry the instant he heard the loud organ, which, being so much more powerful than that to which he had been accustomed at home, he was some time before he could bear without discovering pain, occasioned, perhaps, by the extreme delicacy of his ear, and irritability of his nerves.

The first voluntary he heard with attention was performed at his father's house by Mr. MULLY, a music-master; and as soon as he was gone, the child seeming to play on the organ in a wild and different manner from what his mother was accustomed to hear, she asked him what he was doing? And he replied, "I am playing the gentleman's fine thing." But she was unable to judge of the resemblance: however, when Mr. MULLY returned a few days after, and was asked, whether the child had remembered any of the passages in his voluntary, he answered in the affirmative. This happened about the middle of November 1777, when he was only two years and four months old, and for a considerable time after he would play nothing else but these passages.

A musical gentleman of Norwich informed Mr. PARTRIDGE, that, at this time, such was the rapid progress he had made in judging of the agreement of sounds, that he played the Easter-Hymn with full harmony; and in the last two or three bars of *Hallelujah*, where the same sound is sustained, he played chords with both hands, by which the parts were multiplied to six, which he had great difficulty in reaching on account of the shortness of his fingers. The same gentleman observed, that in making a base to tunes which he had recently caught by his ear, whenever the harmony displeased him, he would con-

tinue the treble note till he had formed a better accompaniment.

From this period his memory was very accurate in retaining any tune that pleased him: and being present at a concert where a band of gentlemen-performers played the overture in *Rodelinda*, he was so delighted with the minuet, that the next morning he hummed part of it in bed; and by noon, without any further assistance, played the whole on the organ.

His chief delight at present is in playing voluntaries, which certainly would not be called music if performed by one of riper years, being deficient in harmony and measure; but they manifest such a discernment and selection of notes as is truly wonderful, and which, if spontaneous, would surprize at any age. But though he executes fragments of common tunes in very good time, yet no adherence to any particular measure is discoverable in his voluntaries; nor have I ever observed in any of them that he tried to play in triple time. If he discovers a partiality for any particular measure, it is for dactyls of one long and two short notes, which constitute that species of common time in which many street-tunes are composed, particularly the first part of the *Belleisle March*, which, perhaps, may first have suggested this measure to him, and impressed it in his memory. And
his

his ear, though exquisitely formed for discriminating sounds, is as yet only captivated by vulgar and common melody, and is satisfied with very imperfect harmony. I examined his countenance when he first heard the voice of Signor PACCHIEROTTI, the principal singer of the opera, but did not find that he seemed sensible of the superior taste and refinement of that exquisite performer; however, he called out very soon after the air was begun, "He is singing in F."

And this is one of the astonishing properties of his ear, that he can distinguish at a great distance from any instrument, and out of sight of the keys, any note that is struck, whether A, B, C, &c. In this I have repeatedly tried him, and never found him mistaken even in the half notes; a circumstance the more extraordinary, as many practitioners and good performers are unable to distinguish by the ear at the opera or elsewhere in what key any air or piece of music is executed.

But this child was able to find any note that was struck in his hearing, when out of sight of the keys, at two years and a half old, even before he knew the letters of the alphabet: a circumstance so extraordinary, that I was very curious to know when, and in what manner, this faculty first discovered itself; and his father says, that in the middle of January 1778, while he was playing the organ, a particular note hung, or, to speak the language

language of organ-builders, ciphered, by which the tone was continued without the pressure of the finger: and though neither himself nor his elder son could find out what note it was, the child, who was then amusing himself with drawing on the floor, left that employment, and going to the organ immediately laid his hand on the note that ciphered^(c). Mr. CROTCH thinking this the effect of chance, the next day purposely caused several notes to cipher, one after the other, all which he instantly discovered: and at last he weakened the springs of two keys at once, which, by preventing the valves of the wind-chest from closing, occasioned a double cipher, both of which he directly found out. Any child, indeed, that is not an idiot, who knows black from white, long from short, and can pronounce the letters of the alphabet by which musical notes are called, may be taught the names of the keys of the harpsichord in five minutes^(d); but, in general, five years would not be sufficient, at any age, to impress the mind of a musical student with an infallible reminiscence of the tones produced by these keys, when not allowed to look at them.

(c) This circumstance proves that he exercised his eye in drawing, after his manner, before he was two years and a half old.

(d) By remarking that the short keys, which serve for flats and sharps, are divided into parcels of threes and twos, and that the long key between every two short keys is always called D, it is extremely easy from that note to discover the situation and names of the rest, according to the order of the first seven letters of the alphabet.

Another

Another wonderful part of his pre-maturity was the being able at two years and four months old to transpose into the most extraneous and difficult keys whatever he played; and now, in his extemporaneous flights, he modulates into all keys with equal facility.

The last qualification which I shall point out as extraordinary in this infant musician, is the being able to play an extemporary base to easy melodies when performed by another person upon the same instrument. But these bases must not be imagined correct, according to the rules of counter-point, any more than his voluntaries. He generally gives, indeed, the key-note to passages formed from its common chord and its inversions, and is quick at discovering when the fifth of the key will serve as a base. At other times he makes the third of the key serve as an accompaniment to melodies formed from the harmony of the chord to the key-note; and if simple passages are played slow, in a regular progression ascending or descending, he soon finds out that thirds or tenths, below the treble, will serve his purpose in furnishing an agreeable accompaniment.

However, in this kind of extemporary base, if the same passages are not frequently repeated, the changes of modulation must be few and slow, or correctness cannot be expected even from a professor. The child is always as ready at finding a treble to a base as a base to a treble,
if

if played in flow notes, even in chromatic passages; that is, if, after the chord of c natural is struck, c be made sharp, he soon finds out that A makes a good base to it; and on the contrary, if, after the chord of D with a sharp third, F is made natural, and A is changed into B, he instantly gives G for the base. Indeed he continued to accompany me with great readiness in the following chromatic modulation, ascending and descending:

I made more experiments of this kind, but to relate them would render my account too technical to all but composers, or such as have long studied harmony.

When he declares himself tired of playing on an instrument, and his musical faculties seem wholly blunted, he can be provoked to attention, even though engaged in any new amusement, by a wrong note being struck in the melody of any well-known tune; and if he stands by the instrument when such a note is designedly struck, he will instantly put down the right, in whatever key the air is playing.

At present, all his own melodies are imitations of common and easy passages, and he seems insensible to others; however,

however, the only method by which such an infant can as yet be taught any thing better seems by example. If he were to hear only good melody and harmony, he would doubtless try to produce something similar; but, at present, he plays nothing correctly, and his voluntaries are little less wild than the native notes of a lark or a black-bird. Nor does he, as yet, seem a subject for instruction: for till his reason is sufficiently matured to comprehend and retain the precepts of a master, and something like a wish for information appears, by a ready and willing obedience to his injunctions, the trammels of rule would but disgust, and, if forced upon him, destroy the miraculous parts of his self-taught performance.

Mr. BAILLET published in the last century a book, *Sur les Enfants celebres par leurs etudes*; and yet, notwithstanding the title of his work, he speaks not of infants but adolescents, for the youngest wonder he celebrates in literature is at least seven years old; an age at which several students in music under my own eye have been able to perform difficult compositions on the harpsichord, with great neatness and precision. However, this has never been accomplished without instructions and laborious practice, not always voluntary.

Musical prodigies of this kind are not infrequent: there have been several in my own memory on the harpsichord. About thirty years ago I heard PALSCHAU, a

German boy of nine or ten years old, then in London, perform with great accuracy many of the most difficult compositions that have ever been written for keyed instruments, particularly some lessons and double fugues by SEBASTIAN BACH, the father of the present eminent professors of that name, which, at that time, there were very few masters in Europe able to execute, as they contained difficulties of a particular kind; such as rapid divisions for each hand in a series of thirds, and in sixths, ascending and descending, besides those of full harmony and contrivance in nearly as many parts as fingers, such as abound in the lessons and organ fugues of HANDEL.

Miss FREDERICA, now Mrs. WYNNE, a little after this time, was remarkable for executing, at six years old, a great number of lessons by SCARLATTI, PARADIES, and others, with the utmost precision.

But the two sons of the reverend Mr. WESTLEY seem to have discovered, during early infancy, very uncommon faculties for the practice of music. CHARLES, the eldest, at two years and three quarters old, surprised his father by playing a tune on the harpsichord readily, and in just time: soon after he played several, whatever his mother sung, or whatever he heard in the street.

SAMUEL, the youngest, though he was three years old before he aimed at a tune, yet by constantly hearing his brother practise, and being accustomed to good music
I and

and masterly execution, before he was six years old arrived at such knowledge in music, that his extemporary performance on keyed instruments, like MOZART's, was so masterly in point of invention, modulation, and accuracy of execution, as to surpass, in many particulars, the attainments of most professors at any period of their lives.

Indeed MOZART, when little more than four years old, is said to have been "not only capable of executing lessons on his favourite instrument, the harpsichord, but to have composed some in an easy style and taste, which were much approved;" and SAMUEL WESTLEY before he could write was a composer, and mentally set the airs of several oratorios, which he retained in memory till he was eight years old, and then wrote them down.

Here the difference of education appears; little CROTCH, left to nature, has not only been without instructions but good models of imitation; while MOZART and SAMUEL WESTLEY, on the contrary, may be said to have been nursed in good music: for as the latter had his brother's excellent performances to stimulate attention, and feed his ear with harmony; the German infant, living in the house of his father, an eminent professor,

(e) See Phil. Transf. vol. LX. for the year 1770; an account of a very remarkable young musician, by the honourable. DAINES BARRINGTON, F. R. S. who soon intends to favour the public with an account of the two WESTLEYS.

and an elder sister, a neat player on the harpsichord, and constantly practising compositions of the first class for that instrument, had every advantage of situation and culture joined to the profusion of natural endowments.

Of MOZART's infant attempts at music I was unable to discover the traces from the conversation of his father; who, though an intelligent man, whose education and knowledge of the world did not seem confined to music, confessed himself unable to describe the progressive improvements of his son during the first stages of infancy. However, at eight years of age I was frequently convinced of his great knowledge in composition by his writings; and that his invention, taste, modulation, and execution in extemporary playing, were such as few professors are possessed of at forty years of age.

Into what the present prodigy may mature is not easy to predict; we more frequently hear of trees in blossom during the winter months than of fruits in consequence of such unseasonable appearances. However, to keep pace with the expectations to which such premature talents give birth is hardly allowed to humanity. It is the wish of some, that the uncommon faculties with which this child is endowed might be suffered to expand by their own efforts, neither restrained by rules, nor guided by examples; that, at length, the world might be furnished with a species of natural music, superior to all the surprizing productions

productions of art to which pedantry, affectation, or a powerful hand, have given birth. But, alas! such a wish must have been formed without reflexion; for music having its classics as well as poetry and other arts, what could he compose or play upon different principles that would not offend the ears of those who have regarded those classics as legislators, and whose souls have been wrapped in elysium by their strains? He might as well, if secluded from all intercourse with men, be expected to invent a better language than the present English, the work of millions, during many centuries, as a new music more grateful to the ears of a civilized people than that with which all Europe is now delighted.

An individual may doubtless advance nearer perfection in every art by the assistance of thousands, than by the mere efforts of his own labour and genius.

Another wish has been formed, that the effects of different genera and divisions of the musical scale might be tried upon this little musician; but the success of such an experiment is not difficult to divine. An uncultivated ear would as naturally like the most plain and common music, as a young mind would best comprehend the most simple and evident propositions: and, as yet, the attention of CROTCH cannot be excited by any musical refinements or elaborate contrivance.

It has likewise been imagined by some, that every child might be taught music in the cradle, if the experiment were made; but to these it may with truth be said, that such an experiment is *daily* made on every child, by every mother and nurse, that is able to form a tune, on every part of the globe. In Italy the *ninne nonne*, or lullabies, are fragments of elegant melodies, become common and popular by frequent hearing; and these, though they help to form the national taste, are not found to stimulate the attention of Italian children to melody, or to accelerate the display of musical talents at a more early period than elsewhere.

Premature powers in music have as often surprized by suddenly becoming stationary as by advancing rapidly to the summit of excellence. Sometimes, perhaps, nature is exhausted or enfeebled by these early efforts; but when that is not the case, the energy and vigour of her operations are seldom properly seconded, being either impeded and checked by early self-complacence, or an injudicious course of study; and sometimes, perhaps, genius is kept from expansion by ill-chosen models; exclusive admiration, want of counsel, or access to the most excellent compositions and performers in the class for which nature has fitted those on whom it is bestowed.



XIX. *Account of a new Method of cultivating the Sugar Cane. By Mr. Cazaud; communicated by Joseph Banks, Esq. P. R. S.*

Read February 25, 1779.

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Vue

THOUGH the work which I have the honour to lay before the Royal Society was undertaken entirely with a view to private advantage, it appeared to me not entirely destitute of public utility, were that utility no other than what may arise from there existing a regular history of a plant which is worth nine millions sterling to Europe.

The usual method of cultivating the sugar cane appears more like the consequence of some general observations than the result of such as ought to have been purposely made.

The methods in use, before mine, are reducible to two.

The

L'OUVRAGE que j'ay l'honneur de presenter et de soumettre au jugement de la Societé Royale de Londres, entrepris uniquement pour des vues particulieres, m'a paru susceptible d'une utilité générale, quand il ne devoit produire qu'une discussion sur une plante qui fournit à l'Europe une reproduction annuelle de neuf millions sterling.

La culture ordinaire de la canne me paroît plutôt être le fruit de quelques observations générales que le resultat de celles qu'on auroit du faire.

On peut reduire à deux methodes, les systemes établis avant celui que je propose.

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L'une

The one consists in making sugar in almost all seasons indifferently, consequently in planting rather (if one may so say) when the planter is best prepared for his work, than at the time which is best suited to the essential end, which undoubtedly should be, *to get from the earth, all that can be expected from its fruitfulness after due allowance has been made for the different circumstances of the growth of the cane, and the revolutions it undergoes in the different seasons.*

This first method arose, from the want of proper observations, and the difficulty of procuring the number of negroes requisite to work in another manner; so that the desire of cultivating a greater quantity of land always having kept pace with the augmentation

L'une consiste à faire du sucre presque indistinctement en tout tems, et conséquemment à planter, s'il est permis de le dire, plutôt dans le tems où l'on est le mieux arrangé pour cette operation, que dans celui qui conviendrait le plus au but essentiel, qui doit être, *de tirer de la terre tout ce qu'on peut espérer de sa fertilité, combinée avec les différentes circonstances de l'accroissement de la canne, et des revolutions qu'elle éprouve dans les différentes saisons.*

Cette methode a été une suite nécessaire de la difficulté qu'on avoit dans le principe de se procurer la quantité de negres qu'il auroit fallu pour travailler autrement, jointe au manque d'observations qui pussent encore diriger. Le désir de cultiver une plus grande quantité de terre augmentant avec le nombre des negres,

tation of the negroes, the latter must of course have been always inadequate to the work; whence the only change such a method of culture could ever admit of consisted in making a little more sugar in a good season, and a little less in a bad one.

The second method, which I call the system of cultivation in the great way, and is that which has been either originally followed or adopted wherever the number of negroes has been answerable to the assurance the slave-merchant had of receiving the price he had agreed to take for them, consists, first, in planting a fourth or fifth of one's land in October, November, and December, when, all the other business of the plantation being finished, there is time to give to this important one all the attention it deserves.

2dly,

negres, dont la quantité se trouvoit toujours ainsi disproportionnée aux travaux, le seul changement que l'avidité d'étendre ses plantations ait pu admettre dans le premier plan, a donc été de faire un peu plus de sucre dans la bonne saison et un peu moins dans la mauvaise.

L'autre methode que j'appelle le systeme de la grande culture a été suivie où adoptée dans tous les pays où la facilité de se procurer des negres a été proportionnée à l'avantage qu'on avoit soin d'assurer au marchand, d'en recevoir le payement aux termes stipulés. Cette methode consiste, 1^o, à planter le quart ou le cinquieme de sa terre en Octobre, Novembre, et Decembre, parce qu' alors tous les autres travaux etant finis, on est tout entier à cette opération importante.

2dly, In digging very deep trenches, because the deeper the trench the greater the nourishment of the root.

3dly, In planting at great distances, by which the air circulates more freely between the plants, and by that means ensures them a quicker and more complete maturity.

Finally, in cutting the canes in the four finest months of the year, to wit, in February, March, April, and May, because the sugar is then the finest, is cut with the least trouble, and (at least as is pretended) is supplied in greater quantities by the canes.

Where this method is followed, about three fourths of the plantation is cut, the remainder is made up partly of young canes, which can only be cut the following year, and partly of such as must necessarily be sacrificed and reserved for the purpose of getting

2°, à faire des fosses très profondes, parce que les racines trouvent plus de nourriture dans une plus grande profondeur.

3°, à planter à de grandes distances, parce que l'air circule mieux entre les plantes, et leur procure une maturité plus prompte et plus parfaite.

4°, enfin, à faire la récolte pendant les 4 mois de la plus belle saison, Février, Mars, Avril, May, parce qu' alors le sucre se fait plus aisément, plus beau, et que les cannes; dit-on, en donnent une plus grande quantité.

On coupe dans ce système environ les trois quarts de la terre destinée aux cannes; le reste est partie en jeunes plantes qu'on ne peut couper que l'année d'après, et partie dans quelque portion de terre sacrifiée nécessairement pour se procurer

getting the plants which are wanted after the crop is over.

My own method, which I now proceed to explain, is this: I employ the whole of the six first months of the year in the business of the crop, and in May and June plant the canes which have been cut in January. This of course induces a necessity of cutting the ratoons at the end of the eleventh instead of the end of the twelfth month, and the planted canes, which should stand fifteen months, at the end of the year: in return, the whole plantation is cut every year.

It is objected, that besides the first loss from cutting the ratoons one, and the planted canes three months before their time, there is a second which arises from what is cut not being sufficiently ripe. These objections I mean to answer by the history of the cane, that of the seasons,
that

procurer le plan dont on a besoin dans une saison où la récolte est déjà faite.

Je propose d'employer en entier les six premiers mois de l'année à faire la récolte, et de planter en May et Juin, les cannes coupées en Janvier, ce qui entraîne la nécessité de couper toujours, les rejettons à onze mois au lieu de les couper à douze, et les cannes plantées à un au lieu de les couper à 15 mois: mais on coupe chaque année toute la terre destinée aux cannes.

On objecte à ce système, outre la perte qui résulte de l'anticipation d'un mois de coupe pour les rejettons, et de trois mois pour les cannes plantées, le défaut de maturité qui suit de cette anticipation; je réponds à ces objections par l'histoire
de

that of the effect of those seasons, and from experience.

After this, as it is an advantageous consequence (though not a fundamental principle) of my method, that I only plant a sixth part of my land every year, and it is objected to this, that there are some soils which will not give so much as a *second* crop, I endeavour to investigate what causes can deprive a plant propagating by the gem of its power of giving shoots *ad infinitum*, and I find none but such arise from a bad method of cultivation.

Having written in the beginning only with a view of instructing my overseer, and by no means with the presumption of adding to the treasure of natural history by the account of a plant which deserves the pen of a BUFFON, my observations (though I flatter myself extended to every

de la canne, des saisons, de leurs effets, et de l'expérience.

Je ne plante aussi annuellement que le sixième de ma terre, mais ce n'est pas l'article fondamental du système, il me paroît seulement en être une suite avantageuse : cependant comme on objecte contre cet article, l'impossibilité présumée, même d'une seconde coupe dans certaines terres, je cherche les raisons qui peuvent anéantir, dans une plante vivace par sa racine, la faculté de rejettonner jusqu'à la confirmation des siècles ; et je ne puis les trouver que dans un mauvais système de culture.

Quoique je n'aye écrit dans le principe que pour instruire mon œconome, et point du tout avec la présomption d'enrichir le dépôt d'histoire naturelle, de celle d'une plante digne de la plume d'un BUFFON, je ne crois pas cependant qu'il y ait un seul point intéressant de cette histoire qui m'ait échappé, mais tous

every material point) were scattered, confused and drawn up in the natural order they offered themselves to me, when the necessity of discussing or defending any part of my method obliged me to study the nature of the plant, to try to find out the reason of some phenomena which the experience of many years had taught me the existence of; but I have been obliged to follow a different method now that these observations are to be submitted to the natural judges of every thing that can have influence upon the progress of useful knowledge. In order to form a proper judgment of the merit of the mode of culture I propose, it is necessary to be acquainted with the plant and the climate in which it grows.

Obfer-

etoient epars, sans ordre, comme ils s'étoient présentés, à mesure qu'il falloit discuter et établir quelque partie de mon système, et que j'étois obligé de chercher dans la nature de la plante, la raison de quelque phénomène que plusieurs années d'expérience m'avoient fait observer. Mon dessein en les rapprochant, étant de les présenter aux juges naturels de tout ce qui peut influer sur le progrès des connoissances utiles, j'ay du suivre une autre methode: pour apprécier la culture que je propose il faut connoître la plante, et le climat où on la cultive.

Obfer-

Observations on the climate.

In the Windward Islands, the weather is commonly dry from the 15th of February to the 15th of May. The rains are moderate till August; they are very copious the two or three following months, and afterwards decrease till February; consequently there is a succession of nine months rain, and of three months dry weather. The annexed table shews the quantity of rain fallen at Grenada in the East quarter from the first of June 1772 to the first of June 1773, and this is the rain of a common year.

*Table**Observations sur le climat.*

Dans les Isles Antilles, du 15 Fevrier au 15 May, il fait ordinairement sec, les pluies sont moderées jusqu'en Aoust, elles sont tres fortes pendant les deux ou trois mois suivans, elles diminuent ensuite jusqu'en Fevrier: voila donc une succession de neuf mois de pluie suivie de trois mois de sec; voici le tableau de la pluie tombée à la Grenade dans la partie de l'est depuis le 1 Juin 1772 jusqu'au 1 Juin 1773; c'est le tableau d'une année commune.

Tableau

*Table of rain fallen in the East quarter of Grenada from
the 1st of June 1772 to the 1st of June 1773.*

In. Tenths of an inch Eng. measure.

June	-	9	3
July	-	13	9
August	-	11	9
September	-	19	0
October	-	12	7
November	-	9	1
December	-	18	8
January	-	9	5
February	-	6	4
March	-	2	6
April	-	0	8
May	-	2	9

Total of inches of rain, 116 0

The

*Tableau de la pluie tombée à la Grenade dans la partie de l'est depuis le 1 Juin 1772
jusqu'au 1 Juin 1773.*

Pou. 10^e de pouce Anglois.

Juin	-	9	3
Juillet	-	13	9
Août	-	11	9
Septembre	-	19	0
Octobre	-	12	7
Novembre	-	9	1
Décembre	-	18	8
Janvier	-	9	5
Février	-	6	4
Mars	-	2	6
Avril	-	0	8
May	-	2	9

Total des pouces d'eau 116 0

The natural history of the cane.

There are but few of the minute particulars I am going to mention, but what are some how or other connected with my method.

The upper part of the cane, commonly called the head, is the best plant that can be used to propagate it, see fig. 7. see likewise fig. 1.; it is the part from *l* to *n*, which in this figure is without its leaves. It is a known fact, that the body of the cane does not come up well, unless there are continued rains from the time of its being put into the ground, till all the shoots are out and pretty strong.

*History**Histoire naturelle de la canne.*

Il est peu des détails minutieux dans lesquels je vais entrer qui n'ait un rapport quelconque avec mon système.

La partie supérieure de la canne appelée communément la tête, est le meilleur plan dont on puisse se servir pour la multiplier, v. fig. 7. v. aussi fig. 1. c'est la partie depuis *l* jusqu'à *n*, elle est dans cette figure dépouillée de ses feuilles: il est reconnu que le corps de la canne ne réussit point à moins d'une pluie continuelle depuis le moment qu'il est en terre, jusqu'au moment où tous les jets sont sortis et assez vigoureux.

Histoire

History of the roots of the sugar cane, and of its productions under ground.

If the plant be put into the ground as soon as it is cut, and the weather happens to be very hot and dry, a fortnight often passes before the eye can discover any alteration in the two or three most promising joints; the first being too hard (see *a* fig. 2.) become immediately dry; the others (see from *y* to *z* in the same figure) are only grafts, and produce nothing, unless when the last happens to shoot at its extremity.

When the dry weather continues, but so as not materially to affect the soil in contact with the inferior part of the plant, one discovers at the end of three weeks, about that part of the joint which lies undermost in the trench,

some

Histoire des racines de la canne et de ses productions en terre.

1^o, Si le plan est mis en terre aussi tôt qu'il est coupé, et qu'il survienne de grandes chaleurs sans pluie, il est souvent quinze jours sans que l'œil découvre la moindre alteration dans les deux ou trois nœuds de la plus grande espérance; les premiers, trop durs, v. *a*, fig. 2. font d'abord desséchés; les autres, herbacés, v. *y* jusqu'à *z*, ne produisent rien à moins que le dernier ne se développe par son extrémité.

Si le sec continue et qu'il ne se fasse pas sentir vivement jusqu'à la terre qui touche la partie inférieure du plan, on découvre après trois semaines à cette partie

some white fibres of about half a line in length and thickness: notwithstanding, however, this beginning of vegetation, the plant still dies, unless it rains, before the soil in contact with these small roots be entirely dry; at least I have always found the earth dry under these small roots, when, in consequence of a long drought, the plant from which they had come was entirely dried up. But I have seen plants, put into the ground as soon as cut, put forth one, and sometimes two sprigs at the end of six weeks, though they had been deprived of rain for above a month. When the plant is somewhat withered, that is, when it has not been planted till some days after having been cut, it thrives sooner when assisted by rain, and dies sooner when deprived of it: the reason I take to be this, it could not wither without losing some part of that moisture which it contains, and which it wanted

du nœud qui touche le fond de la fosse, quelques fils blancs de la longueur et de la grosseur d'une demi ligne: après ce commencement de végétation, le plan meurt s'il ne pleut pas avant que la terre qui touche ces petites racines soit entièrement desséchée, ou du moins j'ay toujours trouvé la terre sèche sous ces petites racines: lorsqu' après un long sec, j'ay trouvé le plan qui les avoit données entièrement desséchées; mais j'ay vu du plan mis en terre aussi-tôt que coupé privé de pluie pendant plus d'un mois donner un et quelquefois deux jets après six semaines: si le plan est un peu fanné, c'est à dire coupé quelques jours avant d'être mis en terre, il leve plus vite s'il est secondé de la pluie, et meurt plutôt s'il en est privé, parce qu'il n'a pu se fanner sans perdre une partie de l'eau qu'il contenoit, dont

wanted to preserve it from the dryness of the surrounding earth, which earth itself also probably sucks up part of its humidity: *I am, therefore, apt to think, though contrary to the opinion of a great many people, that it is best to put the plant into the ground as soon as it is cut.*

2dly, We all along suppose the weather to be tolerably favourable; then, when the small roots, which sprout about the joint when the surrounding earth has its proper degree of moisture, are of the length of a line and a half or two lines, the bud adherent to it swells; a few days after it lengthens horizontally in the direction of the plant, see *b*, fig. 2.; it next describes a spiral, *c*; and at length comes out of the earth like the point of an arrow, *d*. These are what we call the first productions; they are the immediate productions of the plant. The shaft

il auroit eu besoin pour se soutenir plus longtems contre la secheresse de la terre qui l'environne, et qui probablement pompe même une partie de son humidité: je crois donc contre l'opinion de beaucoup de gens qu'il vaut mieux mettre le plan en terre aussi-tôt qu'il est coupé.

2°, Nous supposérons un tems assez favorable; lorsque ces petites racines qui se forment à l'entour du nœud si la terre est par tout humide, sont longues d'une ligne et demie ou deux, le bouton qui luy est attaché paroît gonflé, quelques jours après il s'allonge horizontalement, v. *b*, fig. 2. suivant la direction dans laquelle le plan est couché; ensuite il decrit une spirale, v. *c*, même figure, et sort de terre comme une pointe de fleche *d*. C'est ce que j'appelle les premières productions,

shaft of the arrow then breaks, the two first leaves expand, see *e, e*, fig. 3. and rise from twenty-four to thirty inches high. These are, properly speaking, the leaves of the plant, I shall call them the *external* leaves, though they are in fact inclosed within a quadruple sheath of feminal leaves, see *f, f, f, f*; but these last rise but a very little way out of the ground. I believe, that the joint, to which the last leaf of this external case is fastened, ought to be considered as the beginning of the stem of the plant which divides its lower part (that which produces nothing above ground) from its upper part, which produces things of various kinds; at least, I have never seen any thing produced by this lower part, except these four small leaves, and some roots.

3dly;

tions, qui sont les productions directes du plan, Deux ou trois jours après, le faisceau qui compose cette fleche se developpe, les deux premieres feuilles se deployent, v. *ee*, fig. 3. elles s'elevent jusqu'à 24 et 30 pouces, ce sont les feuilles proprement dites de la plante; je les appellerai *exterieures* quoiqu'elles soient dans leur origine enveloppées dans un quadruple etui de feuilles feminales, v. *f, f, f, f*, même figure, parce que celles cy ne s'elevent que très peu au dessus de la terre: je crois que le nœud auquel est attaché la dernière feuille de cette enveloppe doit être regardé comme le collet de la plante qui divise sa partie inferieure qui ne donne point de productions hors de terre d'avec sa partie superieure, d'où il en sort de toute espèce: je n'ay du moins jamais remarqué dans cette partie inferieure, d'autre production que ces 4 petites feuilles et des racines.

3°.

3dly, Till the external leaves are eight or ten inches high, all the nourishment which the plant receives from the earth is by the roots which have sprouted about the joint to which the bud from whence it was produced was fastened: these may, therefore, be considered as *a set of roots of the first order*, see fig. 5. n. 1.

4thly, But when that time comes, the feminal leaves which formed the sheath, wither, grow red, soon after dry up, and are finally driven away by *a set of roots of the second order*, which appear in the places to which each of these feminal leaves were fastened. The fourth figure shews the young plant deprived of its four feminal leaves, and provided with its set of roots of the second order, see N° 2, which have taken their place.

5thly,

3°, Jusqu'au moment où les feuilles extérieures sont hautes de 8 à dix pouces, la terre ne transmet de nourriture à la plante que par les racines sorties à l'entour du noeud auquel étoit attaché le bouton qui l'a produite, et qu'on peut regarder comme un *premier ordre de racines*, v. F. 5. n. 1.

4°, Mais alors les feuilles féminales qui composoient l'enveloppe, se fanent, rougissent, peu de tems après se dessèchent, et sont bientôt chassées par un *second ordre de racines* qui paroissent à l'endroit où chacune des feuilles féminales étoit attachée, la fig. 4. présente la jeune plante dénuée de ses quatre feuilles féminales et pourvue de son second ordre de racines, v. N° 2. qui a pris leur place.

5thly, Each of the *external* leaves mentioned in the second article (I mean those which appeared first) are likewise so many indications of joints in the ground; so much the more distinct, the more vigorous the leaf is; and in proportion as each of these leaves dries; each of the joints successively produces a *row* of roots which constitute the *third set*, see N° 3. fig. 5.

It is to each of these rows of roots that the joint is fastened which is to furnish the suckers which I call the second productions. The fifth figure shews the plant without either its feminal or its external leaves, the joints of which are in the ground. It appears provided with its first, second, and third set of roots, N° 1, 2, 3. Each row of this third set of roots is intended either to nourish the plant which is to come out of the joint which is fastened
to

5°, Chacune des feuilles *extérieures* mentionnées à l'article 2 (j'entends les premières sorties) annonce aussi un nœud en terre d'autant plus distinct que la feuille est plus vigoureuse, et à mesure que chacune de les feuilles se dessèche, chaque nœud donne successivement un *plan* de racines qui forment le *troisième ordre*, v. N° 3. fig. 5.

C'est à chacun de ces plans de racines qu'est attaché le nœud qui doit donner les jets que j'appelle secondes productions. La figure 5 présente la plante dénuée tant de ses feuilles feminales que de ses feuilles extérieures dont les nœuds sont en terre; elle paroît aussi pourvue de son premier ordre de racines N° 1, de son second ordre de racines N° 2, et du 3° N° 3, chaque plan de ce 3° ordre de racines est destiné à nourrir la plante qui sortira du bouton qui luy est attaché, si
la

to it, if the season be favourable enough, or if either the season or cultivation be unfavourable to the birth of this bud; in that case, the roots are to contribute to the nourishment of the plant already formed.

6thly, Let us now go back a little. Suppose the season always to continue favourable, and the first shoot to be out of the earth a fortnight or three weeks after planting, consequently the *first set* of roots formed.

At five or six weeks end the feminal leaves are expelled by a *second set* of roots. In nine or ten weeks the first of the *external* leaves begins to give way to the first row of the third set of roots.

About the twelfth or fourteenth week the bud which adheres to the joint which forms the first row of the third set of roots swells, opens, and likewise appears like the
shaft

la saison est assez favorable, ou bien à nourrir seulement la plante déjà formée, si le défaut de saison ou de culture s'oppose au développement du bouton.

6°, Revenons sur nos pas, supposant toujours la saison favorable, et le premier jet sorti de terre après quinze jours ou trois semaines, conséquemment le *premier ordre de racines* déjà formé.

A 5 ou 6 semaines les feuilles feminales cèdent au *second ordre de racines*. A 9 ou 10 semaines la première des feuilles *extérieures* commence à céder au premier plan des racines du *troisième ordre*.

A 12 ou 14 semaines le bouton attaché au nœud qui forme ce premier plan de racines du 3^e ordre, se gonfle, se développe et paroît aussi comme une fleche,

shaft of an arrow, see *f*, fig. 5. This is the immediate growth or production of the main stem already formed. The buds which adhere to the second, third, and fourth row of the third set of roots likewise unfold and appear in regular succession during each of the following weeks, if the rains continue. Still, however, these are only *second productions*. The *third* are those which come out of the second in a month or five weeks after, *if the rains continue*, see *g*, fig. 5. But we must not forget to observe, with respect to the *second productions*, that an exceeding favourable season being necessary to their appearance, and this twelfth or fourteenth week (from the circumstance of the plantation, being began in October or November) falling out in February or March, which is the usual time of the greatest droughts, they may be kept

v. *f*, fig. 5. C'est une production propre de la souche déjà formée: les boutons attachés au 2^e, 3^e, 4^e, &c. plan de racines de ce 3^e ordre, se développent aussi et paroissent successivement chacune des semaines suivantes *si la pluie continue*, mais ce ne sont encore que des *secondes productions*. Les *troisiemes productions* seront celles qui un mois ou cinq semaines après sortiront de ces secondes, v. *g*, fig. 5. *si la pluie continue*. Mais ce qu'il ne faut pas oublier par rapport aux *secondes productions*, c'est qu'elles ont besoin pour paroître d'une saison très avantageuse, et que cette 12^e ou 14^e semaine dans laquelle elles doivent paroître ne tombant eu egard aux plantations de Novembre et Decembre, qu'en Fevrier où Mars, tems du plus grand sec qui suspend presque entierement la vegetation dans la terre

7

decouverte,

kept back, and only appear the May or June following, after the rainy season has set in.

The fifth figure represents the bud (*b*) which adheres to the first row of the third set of roots, quite come out, and appearing above ground like the shaft of an arrow *f*. This is the second production. The same figure likewise represents the bud (*c*) unfolded and appearing in *g*. This is the third production. It comes out of the first row of roots of the plant *f*, which must be conceived in a more advanced state of vegetation than it is in the figure.

The time of, and requisites necessary for, the successive appearances of all these different vegetations, seem to me so many proofs of the fundamental part of my method, to wit, *the necessity of planting in the only season fitted to accelerate and preserve them.*

The

découverte, ces *secondes productions* retenues dans la terre ne paroîtront donc qu'en May ou Juin suivant, lorsque les pluies seront décidées.

La figure 5 presente le bouton (*b*) du premier plan du 3^e ordre de racines développé et paroissant comme une pointe de fleche *f* hors de terre que nous supposons *a*. C'est la seconde production. La même figure presente aussi le bouton (*c*) développé, et paroissant en *g*. C'est la 3^e production, elle sort du premier plan de racines de la plante *f* qu'on doit supposer plus avancée qu'elle n'est représentée ici.

Le tems et les conditions du developpement des ces differentes productions me paroissent autant de preuves de la partie fondamentale de mon systeme, sçavoir, *la nécessité de planter dans l'unique saison qui soit propre à hater et entretenir ces developpements.*

The nine months during which you may reasonably look out for a continuation of rain are from the 15th of May to the 15th of February. The progression of the rain keeps, as it were, pace with that of your canes when they are planted in May. Moderate in the beginning, more considerable afterwards, very copious when your canes are big enough to have nothing to fear from them, they lessen gradually as the time of the crop draws near.

7thly, Whenever I stubbed up the stumps of a plant which had been ten months in the ground, I always found a portion of the woody part of the plant very sound: the medullary part was rotten. This made me think that every *planted* cane continued, for the whole time of its duration, to derive its nourishment not only from the roots of the *second* and *third orders* (which may be looked upon

Les neuf mois où l'on peut raisonnablement espérer une continuation de pluie, sont depuis le 15 May jusqu'au 15 Fevrier. Les pluies augmentent pour ainsi dire avec vos cannes plantées en May; foibles d'abord, plus considerables ensuite, et par averfes lorsque vos cannes sont assez grandes pour ne les plus redouter, cessant enfin par degrés à mesure que le tems de la coupe approche.

7°, Lorsque j'ay deraciné des souches qui provenoient d'un plan mis en terre depuis dix mois, j'ay toujours trouvé très saine une portion de la partie ligneuse du plan; la partie medullaire étoit pourrie; et j'ay cru que chaque canne *plantée* continuoit donc pendant qu'elle subsistoit à tirer sa nourriture non seulement du *second* et du *troisième* ordre de racines qu'on peut regarder comme ses racines propres,

upon as its own roots) but likewise from those of the *first*, that is, from those which had sprung about the joint to which the button which produced it was fastened. Fig. 6. represents an entire cane (*a*) sprung from the plant *b*, the woody part *cc*, was as found at the end of ten months as when it was put into the ground: the medullary part was entirely rotted as was likewise the herbaceous, *dddd*, which forms the top of the head. There is no doubt but the medullary part affords the bud the same nourishment as the lobes of almonds do to the germ which they contain.

8thly, Whenever I stubbed up the stumps of a plant which had been thirteen months in the ground, and the canes of which had been cut a month or two before, I always found all the woody part of the plant which belonged to the stump rotten, the roots of the plant dried
up,

pres, mais encore du *premier* ordre de racines, c'est à dire de celles qui s'étoient formées à l'entour du noeud auquel étoit attaché le bouton qui l'a produite. La fig. 6. presente une canne entiere *a* provenant du plan *b*, dont à dix mois la partie ligneuse *cc* étoit aussi saine que lors qu'on l'avoit mise en terre, la partie medullaire étoit entierement pourrie, ainsi que la partie herbacée *dddd* qui compose la partie superieure de la tête: la partie medullaire fournit sans doute au bouton la nourriture que les lobes des amandes fournissent au germe qu'elles contiennent.

8°, Lorsque j'ay deraciné des souches qui provenoient d'un plan mis en terre depuis 13 mois, et dont on avoit coupé les cannes depuis un ou deux mois, j'ay toujours trouvé pourrie toute la partie ligneuse du plan d'ou provenoit la souche,
les

up, each of the young rattoons provided with its *second* and *third* set of roots, and each of these *rattoons* fastened in the ground to the joint of one of the canes which had been cut. This joint being still green, I concluded, from the same kind of reasoning, that these rattoons likewise drew their nourishment not only from their own proper roots, but likewise from those of the joint they had come out of, just as the planted cane draws it both from its own roots, and from those of the joint it comes from.

But as the plant has been always found rotten soon after the cane which it had produced had been cut, I began to suspect, that canes of all sorts, not only the *planted* but the *rattoons* likewise, never draw any nourishment but from

les racines de ce plan desséchées, chacun des nouveaux rejettons en possession de son *second* et *troisième* ordre de racines, et chacun de ces rejettons attaché dans la terre à un nœud d'une des cannes qu'on avoit coupées, ce nœud étant encore verd, j'ay conclu que les *rejettons* jouissoient aussi non seulement de leurs propres racines, mais encore de celles du nœud dont ils estoient sortis, comme la canne plantée jouit de ses propres racines et de celles du nœud dont elle tire son origine.

Mais comme le plan, suivant ce qu'on vient de voir s'est toujours trouvé pourri, peu de tems après que la canne qu'il avoit produite a été coupée, j'ay soupçonné que toutes les cannes non seulement *plantées* mais même *rejettons* ne jouissoient jamais que des trois ordres de racines dont j'ay déjà parlé: ce qui se-
roit

from the three sets of roots of which I have spoken. This, if true, is a strong argument against the pretended antiquity of the stools, which are said to be ten, fifteen, and even twenty years old. It is also a very powerful argument in favour of my principle of the inutility of replanting when the stump is not raised above the ground, since grant my suspicion to be grounded, and it follows, *that the oldest of the roots of a stool, the canes of which are cut every year, cannot be above two years old.*

9thly, In order then to clear up so important a point, I ordered a stool of canes, which had been planted about four years before, had been regularly cut every year, and for the last time about six months before, to be stubbed up. The ratoons were very fine; there were upwards of eighty of them, small and great (hardly a sixth part of this number usually succeeds); the group, notwithstanding-

roit bien contraire à la vétusté pretendue des fouches qu'on dit être de 10, 15 et 20 ans: et seroit un grand argument en faveur de mon principe de l'inutilité de replanter, lorsque la fouche n'est pas élevée au dessus de la terre; puisque dans le cas où mon soupçon seroit juste, *la plus vieille de toutes les racines d'une fouche dont on coupe les cannes tous les ans, n'auroit pas au dela de deux années.*

9°, Pour éclaircir un point aussi important j'ay fait deraciner une fouche de cannes plantées depuis quatre ans, coupée régulièrement chaque année, et pour la dernière fois depuis six mois: les rejettons étoient fort beaux, il y en avoit plus de 80 tant grands que petits (il en reussit rarement la sixième partie) la
tousse

notwithstanding, was not above ten inches in diameter when it came out of the ground. In order not to injure it, I ordered six inches of the surrounding earth to be taken up with it. When I had had it washed and freed from all extraneous earth, I found no vestiges of any wood but that of the canes which had been last cut: I then took each rattoon apart, and found it fastened to a joint of these last canes, which joint was still green. I examined the remains of each of these last canes, and found the kind of bulb, see *u*, fig. 1. by which they had been fastened to some joints of canes of the preceding year, entirely *dried up*. I met with no appearance of this last cane, which consequently had rotted, and in place of it there was nothing to be seen but dry roots; roots take a great while to rot.

Conclude

touffe n'avoit cependant pas au dela de dix pouces de diametre à sa naissance hors de terre; pour ne la point endommager, j'ay fait cerner et enlever avec elle six pouces de terre à l'entour, et après l'avoir lavée et débarrassée de toute sa terre, je n'ay vu de trace d'aucun autre bois que de celui des dernières cannes coupées; j'ay détaché chaque rejetton séparément, et j'ay vu qu'il étoit attaché à un nœud de ces dernières cannes, et que ce nœud étoit encore verd; j'ay suivi les restes de chacune de ces dernières cannes, et j'ay vu entièrement *desséché* l'espece d'ombilic (v. *u*, fig 1.) par lequel ils étoient attachés aux nœuds de quelques cannes de l'année précédente; je n'ay vu aucune apparence de cette ancienne canne, qui par conséquent étoit alors pourrie, on ne voyoit à sa place que des racines desséchées, les racines sont assez long tems à pourrir.

Conclude we then, as I suspected, that the ratoons as well as the planted canes derive nourishment only from the three orders of roots of which I have spoke, with this difference only, that the first order of roots belonging to the *planted canes* comes from the plant which is put under ground; whereas that of the ratoons comes from the part of the cut canes which continue in the ground after they have been cut, and which rots the following year soon after the succeeding ratoons have appeared, so that the third order of roots of a cane which has been just cut becomes the first order of those of the ratoon it is about to produce; which first order, as well as the second, will rot the following year, when new ratoons shall have taken the place of those which shall then be cut.

The

Concluons comme je l'avois soupçonné que les rejettons ne jouissent comme les cannes plantées que des trois ordres de racines dont j'ay parlé, avec cette difference que le premier ordre de racines des cannes plantées provient du plan mis en terre, et que celui des rejettons provient de cette partie des cannes coupées qui reste dans la terre, partie qui pourrira l'année suivante peu de tems après que les rejettons subséquens auront paru; de sorte que le troisieme ordre de racines d'une canne qu'on vient de couper, devient le premier ordre de racines du rejetton qu'elle va produire, lequel premier ordre pourrira l'année suivante ainsi que le second, après que de nouveaux rejettons auront suivi la coupe qu'on aura faite des precedents.

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The idea, therefore, of having stumps of fifteen or twenty years standing must be given up, since it is proved, that the oldest are those of the last year; but this idea being given up, it can never be assigned as a reason for re-planting so often. It will be sufficient, therefore, to replant annually a sixth of one's land: to speak more correctly, it will always be useless to replant, *as long as the stump is not raised above ground, and the ground about it is raised as it ought to be by annual and well-directed labours.* I say, the ground about must be annually raised, for otherwise it is impossible but that the feet of the negroes and cattle must harden and render it impervious to the roots which are to make their way every year.

It likewise appears of what consequence it is to cut the canes in the ground in order to prevent the stump
from

Il faut donc renoncer à la prétention d'avoir des souches de 15 et 20 ans, les plus vieilles sont de l'année précédente, et cette prétendue vétusté des souches étant démontrée fautive, elle ne doit plus être une raison pour replanter aussi souvent; il suffira donc de replanter annuellement la sixième partie de la terre, ou pour mieux dire, il sera toujours inutile de replanter *lorsque la souche ne sera pas élevée au dessus de la terre et que par des labours annuels et bien faits on aura soin de soulever celle qui l'environne*, que les pieds des negres et des bestiaux quand on la travaille ne manqueroient pas d'affaiblir à la longue, et de rendre presque impénétrable aux nouvelles racines qui doivent se former chaque année.

On voit aussi combien il est essentiel de couper les cannes dans la terre, afin
3 d'empêcher

from rising above ground, consequently how bad the method of moulding up the stumps, thought by so many people to be so good, really is; since it raises the stump every year, and ought, therefore, only to be adopted with respect to those pieces one proposes to replant the next year: then, indeed, it is excellent, but in that single case only.

Hence likewise it appears, why rattoons are forwarder at the twelvemonth's end than planted canes are at the end of thirteen. The first order of the roots of the rattoons is in its full vigour at the instant of the crop; that of the planted canes is seldom so at a month's end.

We likewise see why the rattoons of canes that have been cut at the end of ten, eleven, or twelve months, are always finer than those of canes which have stood fifteen or sixteen months; the woody part of the latter being
much

d'empêcher la souche de s'élever; et combien est mauvaise la maxime de se chauffer, que tant de gens trouvent si bonne, qui eleve cependant tous les ans la souche, et qu'on ne devoit suivre que par rapport aux pièces qu'on se propose de replanter l'année suivante, elle est excellente alors, mais dans ce cas seulement.

On voit aussi pourquoi les rejettons a douze mois sont plus avancés que les cannes plantées ne le sont à treize: le premier ordre de racines des rejettons est dans toute sa force au moment de la coupe, celui de cannes plantées l'est rarement au bout d'un mois.

On voit aussi pourquoi les rejettons des cannes coupées a dix, onze, et douze mois sont toujours plus beaux que ceux des cannes coupées a 15 et 16 mois, le

H h 2

bois

much harder is exactly in the same case as the plant, the first joints of which (as I observed before) produced no shoots, except the season happened to be remarkably favourable.

Hence likewise we see, why one never gets any fine rattoons from the grounds which are called exhausted: the planted canes in them are cut at the end of the fifteenth or sixteenth month; at which time the part of their wood which remains under ground, and out of which the rattoons come, is extremely hard. Besides, this is done in February, March, or April, months, as may be seen from the account I have given before of the seasons, not likely to produce miracles of vegetation.

Hence, finally, we see, why it is not at all to be wondered at, that the rattoons which I cut before the time
should

bois de celles cy beaucoup plus dur est dans le cas du plan dont j'ay remarqué cy dessus que les premiers nœuds qui estoient les plus durs, ne donnoient point de jets à moins d'une saison singulièrement favorable.

On voit aussi pourquoy l'on n'a jamais de beaux rejettons dans ces terres qu'on appelle épuisées; on y coupe les cannes plantées à 15. et 16 mois, la partie de leur bois qui reste en terre, et d'ou proviennent les rejettons est alors très dure, et d'ailleurs cette coupe se fait en Fevrier, Mars, et Avril, et l'on peut voir par le tableau de la pluye, p. 217. que ces trois mois ne sont pas propres à produire des miracles de végétation.

On peut conclure aussi qu'il n'est pas etonnant que les rejettons dont j'ay toujours anticipé la coupe aient été (comme je l'ay éprouvé) ceux qui se soient le
5 mieux

should have been what I ever found them to be, those which stood the best; that nothing, on the contrary, so much improves a piece of rattoons as cutting them before the time; and that, therefore, this operation (a necessary one when my method is followed) is not without its attendant advantage, which perhaps much lessens the loss which I have supposed to arise from it in page 213.

10thly, Having washed and gently scraped as far up as their beginning (that is, to the kind of bulb I described) two ratoon canes, six months old; the one taken out of a damp, the other out of a very dry soil; the one with fourteen joints above ground, on a length of twenty-eight inches; the other with only two joints, on a length of two inches; I found that the cane which had fourteen joints above ground, had only five rows of roots of the *third order* on a length of an inch and a half under ground;

mieux soutenus; que rien n'est plus capable de reparer une piece de rejettons que d'en anticiper la coupe; et qu'ainsi l'anticipation des coupes necessaire dans mon systeme, n'est point sans un avantage qui diminue peut-être bien considérablement la perte que j'ay supposée, p. 213.

10°, Après avoir lavé et gratté legerement jusqu'à leur origine (jusqu'à cet espace d'ombilic dont j'ay parlé) deux cannes rejettons de six mois, l'une prise dans un terrain humide, l'autre dans un terrain très sec, l'une ayant hors de terre quatorze noeuds dans une longueur de 28 pouces, l'autre ayant seulement cinq noeuds dans une longueur de deux pouces, j'ay vu que la canne qui avoit quatorze noeuds hors de terre, n'avoit en terre que cinq plans de racine du *troisième ordre* dans une longueur

ground; whereas the cane which had only five joints out of the ground had fourteen rows of roots, likewise of the *third order*, on the same length as the others.

This observation proves, that to the time of the canes jointing, that is, to the appearance of one of its joints above ground, the drying of each external leaf is constantly followed by a row of roots of the third order. One sees too, why canes that are in moist soils joint out of the ground at the three months end; but have then only four or five rows of roots of the third order, distant from each other three, four, and five lines under ground; whereas, on the contrary, in dry lands, or, what comes to the same, in a soil remarkably impoverished for want of cultivation, the canes do not joint above ground before the five months; but then they have under
ground

longueur d'un pouce et demi, et que la canne qui n'avoit que cinq nœuds hors de terre, avoit en terre quatorze plans de racines aussi du *troisième ordre* et dans la même longueur.

Cette observation prouve que jusqu'au tems où la canne noue c'est à dire laisse voir un nœud hors de terre, le dessèchement de chaque feuille extérieure, est toujours suivi d'un plan de racines du troisième ordre; et l'on voit pourquoy dans les terrains humides, les cannes nouent hors de terre à 3 mois, mais alors elles n'ont en terre que 4 à 5 plans de racines du troisième ordre distants les uns des autres de 3, 4, et 5 lignes; dans les terres sèches au contraire, ou (ce qui est égal) singulièrement affaïssées par le défaut de culture, les cannes ne sont nouées hors de terre qu'à cinq mois, mais alors elles ont en terre quatorze à
quinze

ground fourteen or fifteen joints or rows of roots, distant from each other only a line, and sometimes less; the joints, therefore, of the canes, of a dry or ill cultivated soil, are as many in number as those of a moist and fruitful soil; but the two differ in this: the greater part of the former are short, small, and in the ground; whereas the greater part of the last are thick, long, and above ground.

It is therefore the excellence or the defect either of the season, or the cultivation, which keeps back and baltardizes the one sort, which hastens and makes the other thicker: chuse, therefore, that season for planting in which the greatest possible succession of rain shall force out of the grounds those joints which, in a drier season, would only have formed a quantity of roots within it,
the

quinze nœuds ou plans de racines distants les uns des autres d'une ligne seulement et quelque fois moins: les nœuds des cannes d'un terrain sec ou mal cultivé sont donc égaux en quantité à ceux des cannes d'un terrain humide ou fertile, mais ils diffèrent en ce que la plus grande partie des uns sont courts, petits, et dans la terre; le plus grand nombre des autres sont gros longs et hors de terre.

C'est donc la bonté ou le défaut de la culture ou de la saison qui retient et abâtardit les uns, précipite et grossit les autres; choisissez donc pour planter la saison ou la plus grande continuité de pluie possible, forcera le plutôt à se développer hors de terre, les nœuds qui dans une saison plus sèche formeroient uniquement en terre, une quantité considérable de racines dont l'inutilité est visible

the uselessness of which appears from the state of the lands which are burthened with them.

The first reason for planting in May or June (see observation the sixth, p. 227.) respected the coming up of the different productions, first, second, &c. in general; that I have just given regards the first appearances of each particular production. We will next consider if my method is equally favourable to those which are to give the plant all the degree of perfection it is capable of acquiring.

History

visible par l'état des plantes qui en sont affligées.

La première raison de planter en May et Juin, dont j'ay parlé obs. 6. p. 227. intéressoit la sortie des différentes productions en général première, seconde, &c. la seconde raison dont je viens de parler interesse les premiers développemens de chaque production en particulier, voyons si mon système favorise également ceux qui doivent donner à la plante toute la perfection dont elle est susceptible.

History

History of the joints of the cane above ground.

With respect to the joints of the cane, there are two points of equal consequence to be considered, the number and the quality.

With regard to number, the calculation of it, in both the methods, depends upon so many considerations, that I think myself obliged to reduce the question to its most simple state, by barely declaring, that I now cut all my canes every year, instead of only cutting three-fourths of them, as I used to do when I pursued the other method. Those, however, who wish to know more of it, may see in another part of this paper the only calculation the matter is capable of.

As

Histoire des nœuds de la canne hors de terre.

Il se presente à l'égard des nœuds de la canne deux points à considerer aussi essentiels l'un que l'autre, le nombre et la qualité.

Quant au nombre le calcul qu'on peut en faire dans les deux systemes, est subordonné à tant de suppositions que je crois devoir simplifier la question, en exposant que je coupe maintenant toutes mes cannes chaque année au lieu de couper seulement les trois quarts comme je faisois, lorsque je suivois l'autre systeme. Voyez cependant dans son lieu, l'unique espece de calcul dont je croye la matiere susceptible.

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I i

Quant

As to the quality of the joints; this, I think, is to be decided by the history of them, to which hitherto scarce any attention has been paid, though the slightest would have been sufficient to have shewn the absurdity of the received ideas concerning the age which must be allowed the canes, in order for them to arrive at perfect maturity.

At the end of the third month, sometimes later, as this depends upon the soil and season, nothing yet appears but a head covered with leaves: see figure 8. from *a* to *z*, which is a small sheath containing a number of white leaves which come out by degrees. The leaf *b*, see fig. 9. falls at length, and discovers a first joint above ground. From that instant to the time of its full growth it acquires four or five joints every month: these also depend

Quant à la qualité des nœuds il me semble qu'elle peut être décidée par leur histoire à laquelle on n'a fait jusqu'à présent qu'une médiocre attention, quoique la plus légère eût suffi pour démontrer la fausseté des idées qu'on a eues jusqu'à présent sur l'âge qu'il faut donner aux cannes, pour leur procurer une maturité parfaite.

A trois mois quelquefois plus tard (suivant la terre et le tems qu'il a fait) la canne ne présente encore qu'une tige herbacée garnie de feuilles, v. fig. 8. depuis *a* jusqu'à *z* qui est un petit rouleau qui renferme une quantité de feuilles blanches qui doivent se développer par degrés; la feuille *b*, v. fig. 9. tombe enfin et laisse voir un premier nœud hors de terre. De ce moment jusqu'à celui où elle a pris tout son accroissement elle augmente chaque mois de quatre à cinq nœuds

pend upon the weather and soil, and are longer or shorter, thicker or thinner, according as those are more or less favourable. One may see (figure 1, from *i* to *k*) the effects of a continued drought when it sets in in February, and lasts till May or longer. The joints are stunted, and instead of being an inch and a half or two inches long, like those from *b* to *i*, which come out in a favourable season, some of them are hardly two lines, and some even less. When the rains begin again and continue, the joints are longer again (see *k* to *l*) and like the first from *b* to *i*. The effects of the dryness of the soil are exactly similar to those of the weather. I once had a cane in my plantation worthy a place in a collection of natural history: it was of the thickness of a pea, and only three inches long; notwithstanding which

nœuds plus ou moins gros, plus ou moins longs, c'est aussi suivant le tems et le sol, on peut voir fig. 1. depuis *i* jusques à *k* l'effet d'un sec tres vif lorsqu'il commence avec le mois de Fevrier et qu'il continue jusqu'en May ou plus long tems; les nœuds sont etranglés et au lieu d'avoir un pouce et demi ou deux pouces de long, comme les nœuds depuis *b* jusqu'à *i* produits dans une bonne saison, quelques uns de ceux produits dans le tems du sec, ont a peine deux lignes, d'autres en ont moins: lorsque les pluyes recommencent et continuent, les nœuds qui sortent sont plus longs, comme on les voit depuis *k* jusqu'à *l*, et tels que les premiers depuis *b* jusqu'à *i*. L'effet de l'avidité du sol est exactement le même que celui de la vivacité et de la longueur du sec: j'ay vu chez moy dans une espèce de tuff près d'une carrière, une canne digne d'être mise dans le cabinet d'un curieux, elle estoit grosse comme une plume à écrire, longue de trois pouces, et

which it had its two and twenty joints very distinct. I sent it in a letter to a person who might have sent me in exchange some six feet long; which, however, would not have had above twenty-two joints any more than mine; if the first joints of both had come out of the earth at the same time.

The first joint, which comes out either at the third, fourth, or fifth month, always keeps in its first place near the earth, see *a*, fig. 9.; out of this first comes the second, see *b*, fig. 10.; and out of the second a third, after the leaf *cc* has dried, and is fallen off like the leaves *aa*, *bb*. Each week produces its joint, or very nearly: a leaf likewise dries and falls off pretty nearly every week. A cane of thirty-two joints, suppose fit to cut, has from five to eight and twenty of them which have lost their leaves; the next five or six still have them, but withered and

n'en avoit pas moins ses 22 nœuds tres distincts; je l'envoiai dans une lettre à une personne qui auroit pu m'en envoyer en echange quelques autres de 6 pieds de long et qui dans cette longueur n'auroient eu que 22 nœuds comme la mienne si elles avoient noué hors de terre en même tems.

Le premier nœud qui paroît soit à 3 soit à quatre ou à cinq mois, reste toujours à sa même place près de la terre, v. *a*, fig. 9. de celui là sort le second, v. *b*, fig. 10. du second le troisieme, apres que la feuille *c*, *c*, sera desséchée et tombée comme les feuilles *aa*, *bb*, même figure, chaque semaine fournit environ son nœud; il se dessèche et tombe donc à peu près une feuille par semaine; dans une canne de 32 nœuds, je suppose, bons à couper, on en voit 25 à 28 depouillés naturellement de leurs feuilles, et cinq à six autres garnis encore de leurs

and ready to fall off: the remaining joints, surrounded with green leaves, form the head, which one takes care to cut off after the last leaf has withered. The bare recital of facts seems fully to prove that the withering and fall of a leaf is the only criterion (and a sufficient one it is) of the maturity of the joint to which it adhered; and that the eight last joints of two canes, which are cut the same day, have exactly the same age and the same degree of ripeness, notwithstanding one of the canes themselves may be fifteen, and the other only ten months old. My reasoning is further confirmed by the experiment repeatedly made on two equal quantities of canes, cut the same day in pieces of a different age; it was evident, that each joint of the cane, of a supposed growth of ten months, contained the same quantity of sugar as that of a cane of the supposed growth of fifteen.

The

leurs feuilles desséchées prêtes à tomber; les nœuds suivent, garnis de feuilles vertes, forment la tête qu'on a soin de couper après la dernière feuille desséchée: la seule exposition des faits me semble démontrer que le dessèchement et la chute d'une feuille, est l'unique preuve (et preuve suffisante) de la maturité du nœud auquel elle étoit attachée: et que les huit derniers nœuds de deux cannes coupées le même jour sont exactement du même âge et de la même maturité, quand bien même l'une de ces cannes auroit quinze mois et que l'autre n'en auroit que dix; preuve de raisonnement confirmée par des expériences réitérées de deux quantités égales de cannes, prises dans des pièces d'un âge différent, et coupées le même jour, par lesquelles il consiste que chaque nœud de cannes prétendues de dix mois contient autant de sucre que chaque nœud de cannes prétendues de quinze.

La

The necessity, therefore, which my method lays me under of cutting my planted canes at the twelvemonth's end, and my rattoons at eleven, has nothing in it which contradicts the proper idea which ought to be entertained of the absolute maturity of this plant. This is decided by experience, and the history of its joints.

With regard to its relative maturity, that which is of consequence to the sugar, this does not depend on the age, but on the season. In February, March, and April, all the canes, whatever be their age, are as ripe as the nature of the soil ever allows them to be, and accordingly I never fail to make the greatest part of my sugar at this season.

It will be of use to observe, that in the same week in February I cut in a piece which had been planted ten months a quantity of canes sufficient to yield me four thousand

La nécessité ou je suis dans mon système de couper mes cannes plantées à douze mois et mes réjettons à onze, n'a donc rien de contraire à l'idée qu'on doit se faire de la maturité absolue de cette plante, maturité décidée par l'expérience aussi bien que par l'histoire de ses nœuds; quant à sa maturité relative, celle qui intéresse le sucre elle n'est pas l'effet de l'âge mais de la saison, en Février, Mars, et Avril, toutes les cannes de quelque âge qu'elles puissent être, ont la perfection de maturité dont la qualité du terrain les rend susceptibles, et je ne manque pas de faire la plus grande partie de mon sucre dans cette saison.

Il ne sera pas inutile d'observer que j'ay coupé dans la même semaine de Février, dans une pièce âgée de 10 mois, la quantité de cannes nécessaires pour
me

thousand gallons of juice, and exactly the same quantity in a piece which had been planted fifteen months. If any body should chance to repeat this experiment, they should be careful to chuse the same kind of soil, the same exposition, and the same time of year. The juice of the piece which was ten months old was a little greenish, that of the piece of fifteen months was exceeding brown. The four thousand gallons of juice from each of the pieces gave me the same quantity of very fine sugar, which proves that each joint of canes supposed to be nine months old contains just as much sugar as each joint of a cane to be fifteen months old.

It likewise proves, that all the joints, whose leaves have dried and are fallen off, have acquired that maturity which I call absolute.

It

me donner quatre mille galons de *vesou* ou jus, et la même quantité dans une pièceagée de 15 mois (si l'on veut reiteler la même expérience il faudroit choisir dans les deux pièces a peu près la même qualité des terre et la même exposition comme on choisiroit le même tems) le *vesou* de la pièceagée des 10 mois étoit un peu verdâtre, celui de la pièce de 15 mois étoit fort brun; les 4000 galons de *vesou* de l'une et de l'autre pièce m'ont donné la même quantité de très beau sucre. Ce qui prouve que chaque nœud de cannes prétendues de dix mois contient autant de sucre que chaque nœud des cannes prétendues de quinze.

Cela prouve aussi que tous les nœuds dont les feuilles sont desséchées et tombées ont acquis cette maturité que j'appelle absolue.

It likewise proves, that it is not the age of the canes but the dryness of the weather, which increases from January to April, which is the cause that in January four hundred gallons of juice commonly yield forty-eight gallons of sugar and molasses, one with another; in February from fifty-six to sixty-four; in March from sixty-four to seventy-two; in April sometimes eighty; after which period the sugar ferments, and even burns when the refiner happens not to be very expert at his business. Hence I concluded, that the greatest perfection of relative maturity to which my canes could arrive was when the juice of them was made up of four parts water, and one part consisting of part sugar and part molasses.

This last observation likewise proves, that the colour of the juice has nothing to do with either the quantity
or

Cela prouve aussi que ce n'est pas l'age des cannes mais le sec qui augmente de Janvier en Avril qui fait qu'en Janvier 400 gallons de vesou rendent ordinairement 48 gallons tant sucre que melasse, en Fevrier de 56 a 64; en Mars de 64 a 72; en Avril quelquefois 80. Après quoy le sucre mouffe, brule même, si le raffineur n'a qu'une routine; de ce dernier point j'ay conclu que la plus grande perfection de maturité *relative* ou mes cannes pussent atteindre, estoit lorsque leur jus contenoit quatre parties d'eau et une tant de sucre que de melasse.

Cela prouve aussi que la couleur du vesou n'interesse ni la quantité ni la qualité

or the quality of the sugar, but may serve as an indication of the manner in which it is to be made. I should think an object of this importance would not be unworthy the attention of some very great chemist.

Other observations have shewn me that in canes which are perfectly ripe, the quantity of the sugar is equal to that of the molasses.

Let us now examine some other circumstances relative to the growth of the cane, and particularly that of its duration.

The

lité du sucre, mais elle sert d'indice pour la traiter d'une façon ou d'un autre, et il me semble qu'un objet de cette importance ne seroit pas indigne de l'étude d'un très grand chimiste.

D'autres observations m'ont prouvé que dans les cannes parfaitement mures la quantité du sucre est égale à celle de la mélasse.

Examinons quelques autres circonstances de l'accroissement de la canne et sur tout celle de sa durée.

The account of the cane in different soils, together with that of the different degrees and different kinds of growth they give to it.

We are not to form our ideas of what a plant is in a country favourable to it, from what we see it in one where all it reaps from the care bestowed on it, is the lengthening out of a wretched existence useless to every purpose but curiosity. The cane to be met with in the hot-houses of Europe is the bare shadow of the American cane. Let us not then estimate the natural duration of the one by the artificial, and in some measure only apparent duration of the other: I with justice call it an apparent duration; for, if the European cane acquires only

Histoire de la canne dans les différentes espèces de terre, des divers degrés, et de l'espèce d'accroissement qu'elle y acquiert.

Il ne faut pas juger de l'histoire d'une plante dans un climat qui favorise tous ses développemens, par son état dans un pays où elle ne retire d'autre fruit des soins qu'on lui donne, que la prolongation d'une triste existence, inutile à tous autres égards que celui de la curiosité. La canne qu'on voit dans les serres en Europe n'est que l'ombre de celle d'Amérique; ne jugeons donc point de la durée naturelle de l'une, par la durée artificielle et en quelque façon apparente de l'autre; je dis, apparente, car si la canne d'Europe ne croît, je suppose que d'un

only a single joint each year, fifty-two years of existence in Europe are not more than fifty-two weeks existence in America^(a).

R E C I T A L O F F A C T S.

1st, I have never seen, either in my own plantation or that of any other person, a cane which had more than

(a) When I first made this supposition, I rather conjectured what must be than what I knew exactly; but since that time, Mr. THOUIN, of the King's Botanic Garden at Paris, has rid me of the little anxiety I could not help feeling at having ventured an assertion of the truth of which I was not accurately sure. He shewed me a cane which, *he believes*, to have been planted twelve years ago: it has thirty joints. In order to be convinced that it has grown at the rate of two joints and a half a year, one should be fully satisfied that it was planted twelve years ago; but he likewise shewed me one which *he knows* was brought from America in a pot ten years ago, and this has only two joints out of the ground. Conclude we then, as I said before, that, in order to know a plant thoroughly, we should study it in the climate to which it belongs.

forty

d'un nœud par chaque année, 52 années d'existence en Europe n'équivaleroient qu'à 52 semaines d'existence en Amérique *.

E X P O S O N S D E S F A I T S.

1^e observation, Je n'ay jamais vu chez moi ni ailleurs de cannes qui eussent

* Lorsque je hazardois cette supposition je ne faisois qu'imaginer ce qui pouvoit être; Mr. THOUIN, du Jardin du Roy à Paris, m'a mis en état d'apprécier une idée sur laquelle je n'étois pas sans une espèce d'inquiétude; il m'a fait voir une canne *qu'il croit* plantée il y a douze ans: elle a trente nœuds; pour établir qu'elle a cru à raison de deux nœuds et demi par an il faudroit être certain qu'elle a été plantée il y a douze ans; mais il m'a fait voir aussi une canne *qu'il seait* avoir été apportée d'Amerique dans un pot il y a deux ans, elle n'a que deux nœuds hors de terre: concluons du moins comme je l'avoir dit que pour connoître une plante, il faut la suivre dans le climat qui lui est propre.

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forty or fifty useful joints (I do not speak of those of the head); nor have I often met with this number upon any, the length of which fell short of seven, or exceeded nine feet.

2dly, I have never seen this length but either in a new foil, or in a foil the moisteft that could be without being quite drowned; that is to fay, in a foil the most favourable to the quickeft and greateft vegetation of the cane.

3dly, In a foil of this kind I have always seen the first joint out of the ground at the three months end; if there had been frequent showers and no hard rains, at the end of two and a half.

4thly, Even in fuch a foil I have never cut canes at the thirteen months end without finding many of them rotten

au dela de 46 nœuds utiles (je ne parle pas de ceux de la tête) et j'ay rarement vu cette quantité de nœuds dans une longueur moindre de sept pieds, et plus confiderable que neuf.

2^e obfervation, Je n'ay jamais vu cette longueur ailleurs que dans un terrain neuf, ou le plus humide fans être noyé, c'est à dire dans le terrain le plus favorable au plus prompt et au plus grand accroiffement de la canne.

3^e obfervation, Dans un terrain de cette efpèce, j'ay toujours vu la canne nouée hors de terre, à trois mois, quelquefois à deux et demi, s'il y avoit eu des ondées frequentes fans averfes.

4^e obfervation, Je n'y ay jamais coupé les cannes à 13 mois fans en trouver beaucoup de pourries ou prefque deféchées, pourries et couchées, fi l'année avoit été

rotten or almost dried up; rotten and laid if there had been much rain in the year; almost withered, though still standing, if there had been very little.

Is it not safe to infer from these four observations, that forty-six joints are almost the *ne plus ultra* of the growth of the cane in moist soils? and again, that they are the produce of pretty nearly an equal number of weeks which elapse after the appearance of the first joint? for it is impossible to think, that the canes continue growing till the time in which one finds many of them rotten; but if I am in the right it follows, that canes of the first production, which live after the twelfth or at most after the thirteenth month, live as men do who have past the age of forty, every day takes something from their vigour. The second productions are the

été pluvieuse, presque desséchées quoique sur pied, s'il y avoit eu très peu de pluie.

Ne peut on pas conclure de ces quatre observations que ces 46 nœuds font à peu près le *ne plus ultra* de l'accroissement des cannes dans les terrain humides, que ces 46 nœuds étoient le travail d'à peu près autant de semaines qui s'étoient écoulées depuis la sortie du premier nœud, puis qu'on ne peut supposer que les cannes croissent jusqu'au moment où l'on en trouve beaucoup de pourries, et qu'ainsi l'on peut croire que les cannes de la première production qui subsistent encore après douze ou tout au plus 13 mois se soutiennent comme font les hommes après avoir atteint l'âge de 40 ans, tous les jours avec quelque diminution.

the only ones which are then in their perfection; the third and fourth have not yet reached it.

5thly, In a good soil, having a favourable exposition, well drained and worked for a number of years, I have never seen any canes with above thirty-eight or forty joints; nor have I ever seen any that had so many when the length of the cane has fallen short of four feet and a half, nor this except in pretty good years.

6thly, I have never seen the first joint of the cane appear in such a soil sooner than the fourth or the middle of the third month.

Nor, seventhly, have I ever cut such canes at the fourteen or fifteen months end, but I found some that were either rotted or dried, according to the season; rot-
ted

tion de leur valeur réelle: les *secondes productions* sont les seules qui subsistent alors dans leur perfection; les *troisiemes et quatriemes* ne l'ont pas encore atteinte.

5^e observation, Dans un bon terrain bien exposé, egouté, et travaillant depuis plusieurs années, je n'ay jamais vu de cannes qui eussent au dela de 38 à 40 nœuds, ny cette quantité de nœuds dans une longueur moindre de 4 pieds et demi, et ces 4 pieds et demi avec leur 40 nœuds seulement dans des années assez favorables.

6^e observation, Je n'ay jamais vu la canne nouée dans cette espece de terrain avant le 4^e mois ou le milieu du 3^e.

7^e observation, Je n'y ay jamais vu couper les cannes à 14 ou 15 mois sans en trouver de pourries ou desséchées suivant la saison, pourries si je les coupois après
les

ed when I cut them after the January rains; dried when I let them stand the three dry months, and cut them in April.

May one not infer from the fifth and sixth observations, that from thirty-eight to forty joints are nearly the *ne plus ultra* of the growth of the cane in a good soil, with a favourable exposition? and that these thirty-eight to forty joints are the produce of a like number of weeks which elapse from the appearance of the first joint, at the fourth month's end, or a fortnight sooner? consequently, that the only difference between the growth of the canes in such a soil, and what it would have been in a moist one, is only that the former joint four or five weeks later than the last would have done.

And

les pluyes en Janvier, desséchées si je les coupois après trois mois de sec en Avril.

Ne peut-on pas conclure de la 5^e et 6^e observation que les 38 à 40 nœuds sont à peu près le non plus ultra de l'accroissement des cannes dans un bon terrain bien exposé et egouté, que ces 38 à 40 nœuds sont le travail de 38 à 40 semaines écoulées depuis la sortie du premier nœud à 4 mois, ou 3 mois et demi; et qu'ainsi, l'accroissement des cannes dans un pareil terrain ne diffère de celui qu'elles prendroient dans un terrain humide que des quatre ou cinq semaines qu'elles nouent plutôt dans celui ci que dans l'autre.

And may not one infer from the fifth, sixth, and seventh observations put together, that in a good soil, with a favourable exposition, and well drained, the canes of the first production may live the fourteenth month through, perhaps, if they have got to the fourteenth in February, and the dry weather be not excessive, reach to the sixteenth? but that their existence from thirteen months to fifteen will be like that of a stout hale man between forty and five and forty, who is much mistaken if he thinks himself as strong then as he was at thirty-five? Whence it follows, that it must be advantageous to cut the canes, which grow in such a soil, at the twelve month's end, if the other circumstances of the method which has been adopted allow it; or even at the end of eleven months, if these circumstances require it: because in this last case one may expect a compensation

Et ne peut on pas conclure de la 5^e, 6^e, et 7^e observations réunies que dans un bon terrain bien exposé et egouté les cannes de la premiere production peuvent bien se soutenir jusqu'à 14 mois peutêtre même jusqu'à 15, si elles ont atteint le quatorzieme en Fevrier et que le sec ne soit pas bien vif, mais qu'il en sera de leur etat depuis 13 mois jusqu'à quinze, comme de celui d'un homme sec et nerveux depuis 40 jusqu'à 45 ans, qui s'abuse s'il croit être aussi vigoureux qu'à 35. D'où il suit qu'il ne peut être qu'avantageux de couper dans un pareil terrain les cannes à douze mois si les autres circonstances du système qu'on aura adopté peuvent le permettre; et même à 11 mois, si les autres circonstances l'exigent; parce que dans ce cas on peut espérer un dedommagement dans la vigneur

tion in the additional vigour given to the stump, for the disadvantages mentioned above by thus cutting before the usual time.

Observation the 8th, In a dry but good soil, not manured, but well worked and seconded by the season, I have never seen canes with more than from thirty to thirty-four joints, and these on a length of from three to four feet.

Observation the 9th, I have never seen the first joint of these come out sooner than at the end of four months or four months and a half.

Observation the 10th, When I have cut these at the fifteen months end, I have always found them standing, but very dry, and sometimes a little changed.

May we not infer from the eighth and ninth observations, that from thirty to thirty-four joints is nearly the

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vigueur qu'on procure à la souche par une coupe anticipée.

8^e observation, Dans un terrain sec, quoique bon, point fumé, mais bien travaillé et très aidé de la saison, je n'ai jamais vu de cannes qui eussent au delà de 30 à 34 nœuds, et cette quantité dans une longueur de trois à quatre pieds.

9^e observation, Je n'y ai jamais vu le premier nœud sortir avant quatre mois, 4 mois et demi.

10^e observation, Lorsque j'ai coupé ces cannes à 15 mois, je les ai toujours trouvées sur pied, mais très sèches et quelquefois un peu altérées.

Ne peut-on pas conclure de la 8^e et 9^e observation que ces 30 à 34 nœuds sont
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ne plus ultra of the growth of the canes in a foil which is good, though dry; not manured, but pretty well worked, and seconded by the season?

And may we not infer from the three observations put together, that these thirty to thirty-four joints are the work of as many weeks which have elapsed since the appearance of the first joint at the end of four months or four months and a half? consequently, that the growth of the cane in such a foil is nearly equal to what it would be either in a moist or in a well-manured foil, having a good exposition, with this difference however, that the moister the foil is, the quicker the canes joint, and the thicker and longer the joints are: still, however, all the real growth of any consequence takes place in the space of from eleven to twelve months.

Observation

à peu près le non plus ultra de l'accroissement des cannes dans un terrain bon, quoique sec, point fumé, mais assez bien travaillé et aidé de la saison.

Et ne peut-on pas conclure des trois observations réunies, que ces 30 ou 34 nœuds sont l'ouvrage d'autant de semaines écoulées depuis la sortie du premier nœud à quatre mois quatre mois et demi, qu'ainsi l'accroissement des cannes dans un pareil terrain, est à peu près égal à celui qu'elles prendroient dans un terrain humide ou dans un autre bien exposé et egouté, avec cette différence cependant que les cannes nouent d'autant plus vite que le terrain est plus humide, et que dans ce cas les nœuds sont plus gros et plus longs, mais qu'enfin tout l'accroissement réel se trouve renfermé dans l'espace d'environ douze à 13 mois.

Observation the 11th, In a soil which is drier and more parched, particularly in one where tilling or the season have not a little made up for the disadvantages of the exposition and the soil, I have never seen canes with more than from twenty-four to twenty-eight joints; but this number I have seen even on canes which were not more than two feet high.

12thly, I have never seen the cane joint earlier than the end of the fifth month in this sort of soil.

13th, Whenever, after having attempted to cut these canes in January, at fifteen months, I have waited for them till April, in consequence of not being satisfied with their appearance, I have not it is true found one of them rotted, but I have found many dried up.

14th,

11^e observation, Dans un terrain plus sec, plus aride, et sur tout si le travail ou la saison n'ont pas un peu balancé le desavantage de l'exposition et du sol, je n'ai jamais vu de cannes qui eussent au delà de 24 à 28 noeuds, et j'ai vu cette quantité de noeuds même dans des cannes qui n'avoient que deux pieds de haut.

12^e observation, Je n'ai jamais vu dans un terrain de cette espèce la canne nouée avant cinq mois.

13^e observation, Lorsqu'ayant voulu couper les cannes en Janvier à 15 mois, et n'ayant pas été satisfait de leur état actuel, je les ai attendues jusqu'en Avril, je n'en ai pas trouvé à la vérité une seule de pourrie, mais j'en ai trouvé beaucoup d'entièrement desséchées.

14th, When canes planted in the same kind of soil have been either of twelve or fifteen months growth in March or April, and that, either unwilling or unable to cut them then, I have waited till May or June, I have always found many dead stumps, the first productions entirely dried up in those which had stood out best, and the second so changed that it has been with great trouble and great attention that I have been able to make sugar of them.

May not one infer from the eleventh, thirteenth, and fourteenth observations, that from twenty-four to twenty-eight joints are the greatest growth which canes can take in such a soil?

And may one not infer from the twelfth observation, that these joints are the produce of an equal number of weeks,

14^e observation, Dans ce même terrain, lorsque les cannes ont eu dans le mois de Mars et d'Avril soit 12 soit 15 mois, et que je n'ay pas voulu ou pu les couper alors, j'ai trouvé au mois de May et de Juin suivant, beaucoup de souches mortes, j'ai trouvé dans celles qui avoient résisté, le plus grand nombre des *premières* productions totalement desséchées, et les *secondes* si altérées par le soleil, que ce n'a été qu'avec beaucoup de peine et d'attention qu'on a pu en faire du sucre.

Ne peut on pas conclure de la 11^e, 13^e, et 14^e observation que ces 24 à 28 nœuds des cannes dans cette espèce de terrain sont le non plus ultra de l'accroissement qu'elles peuvent y prendre.

Et ne peut on pas conclure de la 12^e, que ces 24 à 28 nœuds sont l'ouvrage d'autant

weeks, which being added to the five months or five months and a half, which elapsed before the appearance of the first joint, make only twelve or thirteen months of real growth, both for the canes which are planted in a dry soil, and for those which are planted in a moist one, the difference only, as I observed higher, there is between the length and thickness of the joints excepted?

15th observation, When good care has been taken to manure and work these dry places, I have always seen the productions of them every way equal to those which had come from a soil well exposed and well drained.

16th observation, When heaps of dung have been laid upon particular parts of such a soil, in order to distribute them thence throughout the remainder of the plantation, I have always seen the canes, grown in which had those places which had been covered with the dung, rotted or dried

d'autant de semaines qui jointes au cinq mois, cinq mois et demi écoulés avant la sortie du premier nœud ne font que douze à 17 mois d'accroissement réel dans les canes du terrain le plus humide; réservé comme j'ai dit plus haut la différence de longueur et de grosseur des nœuds.

15^e observation, Lorsqu'on a eu soin de travailler parfaitement et de bien fumer ces endroits arides, j'y ai toujours vu les productions égales en tout à celles d'un bon terrain bien exposé et egouté.

16^e observation, Lorsqu'on a fait des tas de fumier sur quelques parties de ce terrain sec et aride pour les distribuer dans le reste de la pièce, j'ai toujours vu les canes dans ces petits espaces qui avoient été couverts de fumier, pourries

ou.

dried (according to the season) at the thirteen months end, as the canes of the moist soil were. This difference, however, I found between the two sorts when both were cut at the twelvemonths end; the sugar which was made of the canes that had grown on the soil that was too much dunged burned in April; whereas that made from the canes of the moist soil was finer then than at any other season.

17th, I have seen the effects of these heaps of dung ten years after. The part which had been covered with it gave excellent productions at that distance of time.

May we not conclude from the fifteenth and sixteenth observations, that it is always the fault of the planters, and never that of the plant, if it happens that the cane is not fit to cut at the twelve months end? Now, sup-
posing

ou desséchées, suivant la saison, à 13 mois, comme les cannes du terrain humide; j'y ai cependant observé cette différence qu'en les coupant les unes et les autres à douze mois, le sucre des cannes du terrain trop fumé bruloit en Avril, et que celui des cannes du terrain humide, se faisoit alors plus beau qu'en aucune autre saison.

17^e observation, j'ai vu, même après dix ans, l'effet de ces tas de fumier, et tous les espaces sur lesquels ils avoient été mis donner encore d'excellentes productions.

Ne peut-on pas conclure de la 15^e et 16^e observation que c'est toujours la faute du cultivateur et jamais celle de la plante si la canne n'est pas bonne à couper

posing that the canes planted in October and November are fit to be cut at the end of the twelvemonth, the rains which fall at that time prevent the making any sugar, unless one chuses to make it execrable, and kill all one's cattle. I say nothing of December plantations, because, except in very highly favoured soils indeed, it is rare that one plant in ten succeeds, the dry weather is too near.

May we not infer from the seventeenth observation, that it is likewise the fault of the planters, and by no means that of the soil, if the latter does not afford excellent productions, that is, in other words, good rattoons at the sixth cutting?

Finally, may we not infer from the history of the cane in all kinds of soils, that if there be any in which it

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couper à 12 mois; or en supposant que les cannes plantées en Octobre et Novembre fussent bonnes à couper douze mois après, la pluie qui tombe alors en abondance ne permettroit pas de faire du sucre à moins qu'on ne voulut le faire abominable et écraser les bestiaux. Je n'ai point parlé ici des plantations de Decembre parce qu'il est rare que dans les terrains qui ne sont pas privilégiés, il en réussisse deux ou trois sur dix, le sec est trop voisin?

Ne peut-on pas conclure de la 17^e observation que c'est aussi la faute du cultivateur, et point du tout celle du terrain, s'il ne donne pas encore d'excellentes productions, c'est à dire en d'autres mots, de bons rejettons, à la sixieme coupe.

Et ne peut-on pas enfin conclure de l'histoire générale de la canne dans toutes les espèces de terrains, que s'il en est, ou elle peut subsister jusqu'à 15 et 16 mois,

can exist till the fifteenth or sixteenth month, it never grows to any kind of purpose in any after the thirteenth? Perhaps the accuracy with which I have followed it may authorize me to say boldly after the twelfth.

History of a singular revolution in the inside of the cane, and of the arrow which comes out in consequence of that revolution, and constitutes the last stage of the plant's existence.

A principal and necessary effect of the dry weather on the cane is, the diminution of the watery parts of its juice. There is no reason, therefore, for being surprized, either that the same quantity of canes which affords from 140 to 160 gallons of it in January should only afford 90 to 100 in April, or that there should be as much

mois, elle ne croît cependant jamais d'une façon utile au delà de 13 ? peut être même dirai-je affirmativement douze, après l'avoir suivie jusques dans son dernier développement.

Histoire d'une révolution singulière dans l'intérieur de la plante; de la fleche qui la suit, et qui forme le dernier développement de la canne.

L'effet principal et nécessaire du sec sur la canne, est la diminution de la partie aqueuse de son jus; l'on ne sera donc point surpris que la même quantité de cannes qui en donne au mois de Janvier 140 à 160 gallons, n'en donne que 90 à 100

much sugar made from the latter as from the former. This follows of course; but it should seem, that upon the return of the rains, the cane should recover the water, which the heats of February, March, and April, had made it lose: the reverse, however, is what in fact happens; not only the quantity of the juice is less, but the quality of it is worse; it contains less sugar in it, and when the weather is fairly settled, when the rains are frequent and abundant, which generally happens from the first of July to the 13th of August, the whole vegetative faculties of the plant seem employed in the production of the arrow, see *oop*, fig. 1. During that time the body of the cane is almost totally destitute of juice, which is carried in great plenty towards the head of it: Soon after there comes out from the top of the arrow, a thin

à 100 dans le mois d'Avril, n'y que ces 90 gallons d'Avril donnent autant de sucre que les 160 de Janvier: mais il sembleroit qu'au retour des pluies, la canne devoit recouvrer l'eau qu'elle a perdue par le soleil constant de Fevrier, Mars, et Avril; c'est le contraire qui arrive; non seulement le jus diminue, mais sa qualité se deteriore, il contient moins de sucre, et lorsque le renouveau est tout à fait décidé, que les pluies sont frequentes et abondantes, ce qui arrive ordinairement du 1 Juillet au 15 d'Aoust, il semble que tous les efforts de la vegetation soient consacrés à la production de la fleche, v. *oop*, fig. 1. Le corps de la canne est alors presque totalement destitué de son jus qui se porte en abondance vers la tête de la plante; bientôt il sort de son extremité une tige

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thin pointed stem, which grows about three feet high in the space of five or six weeks, then blows at the end, and is crowned with a panicle like that of a reed. *oop*, fig. 1. shews the arrow crowned with its panicle; it is commonly full-blown towards the end of October, withers soon after, and falls in the following month. Some people pretend, that this panicle contains the seed of the cane; I have sown the kind of seed or dust which comes from it, and nothing has come up. Perhaps the cane which is propagated by shoots bears only empty husks. Be this as it may, when once the arrow has appeared, the cane is in its last stage.

After the fall of the leaf, which takes place a little sooner or a little later, according to the season, the joints which preceded it dry up, in their order. This mortification,

assez grêle et pointue qui s'élève dans l'espace de cinq à six semaines jusqu'à la hauteur d'environ trois pieds, se développe enfin par sa pointe et se couronne d'un panache semblable à celui du roseau. *oop*, fig. 1. présente la fleche couronnée de son panache; elle est ordinairement tout à fait développée à la fin d'Octobre, peu après elle se dessèche, elle tombe le mois suivant. Quelques personnes prétendent que ce panache contient la semence de la canne, je n'en fais rien, mais j'ai semé l'espece de poussière ou de graine qui en sort, et rien n'a levé, peut-être que la canne qui vient de bouture ne porte que des graines folles. Quoiqu'il en soit, après que la fleche est sortie, la canne est à son dernier période; et un peu plus tôt, un peu plus tard, après la chute de la feuille (toujours suivant la saison) les nœuds qui la precedent se dessèchent successivement, et cette

tification, if I may so call it, never stops, except when there happens to come out a kind of spurious top about the eighth or ninth joint from the arrow, see *l, m*, fig. 1.

It comes out of two joints which blow after the fall of the arrow, when there happens to be much rain. This spurious head serves much the same purposes as the true one; for it assists, maintains, and preserves the action of the sap in that part of the cane which still remains alive, so that whenever there is not rain enough to produce it, the last joints die off like the former.

It is farther to be observed with regard to the arrow, 1st, that this any more than the ripeness does not depend upon the age of the cane, but upon the season. In October and November the canes of six months growth bear arrows like those of twelve or fifteen months, but all canes do not bear arrows.

2^d, Though

cette gangrene, si j'ose m'expliquer ainsi, n'est arrêtée que lorsqu'il se forme une espèce de fausse tête vers le 8^e et 9^e avant dernier nœud, v. *l, m*, fig. 1.

Ce sont deux nœuds qui se développent après la chute de la fleche, s'il y a beaucoup de pluie, et qui forment cette fausse tête qui fait en partie l'office de la première, puis qu'elle entretient l'action de la sève dans la partie de la canne qui reste encore saine, et que s'il ne fait pas assez de pluie pour que ces deux nœuds se développent, tous se dessèchent successivement jusqu'à la racine.

Observons encore par rapport à la fleche, 1^o, qu'ainsi que la maturité, elle n'est point l'effet de l'âge de la canne, mais de la saison; dans le mois de Septembre ou d'Octobre les cannes de six mois *flechent* comme celles de 12 et de 15; mais toutes les cannes ne sont pas susceptibles de ce phénomène.

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2^o, Que

2d, Though some canes have no arrow, they are all subject to that internal revolution of which I have spoken, *viz.* the diminution and impoverishment of their juices.

3d, Whenever in a plantation, the foil of which, though good, had been accidentally abandoned, there has happened to be found a shrub strong enough to serve as a prop to a cane having an arrow and shoots from its eighth or ninth joint (*l, m, fig. 1.*); these shoots have each of them produced a cane: far inferior indeed in beauty to that out of which they had themselves sprung, but which, doubtless, would have wanted nothing to have born an arrow and canes in their season also, but such another shrub as that to which they owed their existence.

I know nothing of the observations upon which the opinion is grounded, that the cane does not come to perfection

2°, Que les cannes qui ne flechent pas n'en éprouvent pas moins la révolution intérieure dont j'ay parlé, cette diminution et appauvrissement de leur jus.

3°, Que lorsqu'il s'est trouvé dans une pièce de cannes abandonnée (bonne terre cependant) quelque arbrisseau qui a pu servir d'appuy à une canne flechée et aux jets sortis de son 8^e et 9^e nœud (*l, m, fig. 1.*). Ces jets ont donné chacun une canne beaucoup moins belle à la vérité que celle dont ils sortoient, mais qui n'auroit sans doute eu besoin pour flecher dans son tems, et donner ensuite d'autres petites cannes, que d'être appuyée comme celle qui l'avoit produite.

J'ignore les observations d'après lesquelles on peut prétendre que la canne a besoin

fection under fourteen or fifteen months. Those of which I have been giving an account seem very positively to establish that, *considered with regard to its greatest usefulness, the cane is an annual plant producing by the root*; consequently, the mode of cultivation which agrees best with it, that which will answer best to the planter, will be that which, considering these as the two capital points to be attended to, connects them the best with the necessary influence the seasons of the climate in which the cane grows will have upon it. Now I trust that in these two respects my method will stand the test of examination.

It remains to speak of the history of the cane according to the two methods.

History

besoin de 14 à 15 mois pour être dans sa perfection: celles que je viens de présenter me paroissent établir d'une façon bien positive que *relativement à son plus grand degré d'utilité la canne est une plante annuelle, et vivace par sa racine*, qu'ainsi le système de culture qui lui convient, le système le plus avantageux au cultivateur sera celui qui aura ces deux points pour base, et qui les combinera le mieux avec les effets qui doivent opérer sur elle, les différentes saisons qui règnent dans les climats où on la cultive, et j'ose croire que mon système, peut soutenir l'examen à ces deux égards.

Il me reste à parler de l'histoire de la canne dans les deux systèmes.

Histoire

History of the cane according to the two different methods of cultivation, and in different years, the favourable, the dry, and the rainy.

The history of the cane, according to both methods, flows so naturally from what I have said above, as well of the seasons as of their influence on the different productions, first, second, and third, that the bare recollection of the time and conditions of their appearance and developement will be sufficient to lead us to conclude,

1st, That in favorable seasons, the *first* productions of canes planted in October and November, and cut fifteen months after, have a greater number of joints than those of canes planted in May, and cut the year after at the end of

Histoire de la canne dans les deux systemes, et dans les différentes espèces d'années, favorables, sèches et pluvieuses.

L'histoire de la canne dans les deux systemes decoule si necessairement de ce que j'ai dit cy dessus tant à l'égard des saisons, que de leur influence sur les différentes productions *premieres*, *secondes*, et *troisiemes*, qu'il suffit de se rappeler le tems et les conditions de leur sortie et de leurs developpements, pour conclure :

1^o, Que dans les années favorables, les *premieres* productions des cannes plantées en Octobre et Novembre, coupées quinze mois après ont plus de nœuds que les *premieres* productions des cannes plantées en May, et coupées l'année d'après

of twelve months; but that this excess is only equivalent to the vegetation of one month, on account of the dry weather, which necessarily impedes it during the months of March and April. As to the *second* productions, they are equal in the two methods, those upon the larger scale of cultivation appearing in June to be cut in February, and those of the smaller appearing at the end of August to be cut in May.

2d, The loss in years of drought must be a little more considerable on the small plan than on the other; because there being no *second* productions (of real value) in that case according to either of the plans, and the difference of the thirteenth month, with regard to the *first*, remaining entire, one on thirteen must of course be more felt than one on a greater number.

3d, In

d'après à 12 mois, mais que ce plus n'équivaut qu'à un mois de végétation, attendu l'effet du sec qui dans une terre découverte, la suspend visiblement pendant les mois de Mars et d'Avril; et que les *secondes* productions sont égales dans les deux systèmes; les *secondes* productions de la grande culture ne paroissant qu'en Juin pour être coupées en Février, et les *secondes* productions de la petite culture paroissant à la fin d'Aoust pour être coupées en May.

2°, Que dans les années sèches, la perte doit être un peu plus sensible dans le système de la petite culture, parce que dans l'un n'y dans l'autre système on ne verra paroître de secondes productions (d'une valeur réelle) et que la différence du treizième mois sur les *premières* demeurant en son entier, un est plus sensible sur treize que sur un nombre plus considerable.

3°, Que

3d, In rainy years the *second* and *third* productions will be the same, according to both the methods, on account of the reason given above; to wit, the equality of the time which elapses between their first appearance and the time of their being cut; and because the advantage which the great method might expect from its *first* productions will be lost by their being rotten at the time of cutting.

Finally, that the total difference in the produce of a number of years taken together, cannot be considerable enough not to be much more than made up for by the produce of my second crop, or rather by cutting the whole land, instead of cutting only three parts of it.

Notwithstanding the strong reasons I have to determine me to the choice of the small plan, I confess the
force

3°, Que dans les années pluvieuses, les *secondes* et *troisiemes* productions seront egales dans les deux systemes par la raison que nous avons donnée cy dessus de l'egalité du tems qui s'écoule entre la sortie et la coupe des unes comme des autres, et que le benefice que le systeme de la grande culture pourroit espérer de ses *premieres* productions se trouvera en pourriture au tems de la coupe.

4°, Enfin que la différence totale sur le produit des années combinées ne peut être assez considerable pour n'être pas beaucoup plus que compensée par le produit de ma seconde recolte, ou plutôt par une recolte sur toute la terre au lieu d'une recolte sur les trois quarts.

Malgré l'evidence des raisons qui devoient me decider à suivre le systeme de la petite culture j'avoue que la force du vieux prejugué m'a tenu moy même très long

force of old prejudices long held me in suspense. I think, however, I may with confidence propose it, as I have the experience of five years to bear me up in asserting that, one year with another, there is a difference of above one sixth in its favour.

E X P L A -

long tems en suspens, mais je crois devoir le proposer lorsque je puis prouver par une expérience de cinq années consécutives une différence de quelque chose de plus qu'un sixieme en sa faveur, une année prise dans l'autre.

EXPLANATION OF THE FIGURES.

Figure the first shews a cane entirely without leaves.

N° 1. Is the first order of roots; they come out of the joint to which the bud which is to produce the cane is fastened. All they stand in need of to make them come out is the moisture which is contained in the medullar part of the plant, and the little fermentation which arises from the want of circulation after the two extremities *y* and *z* have been cut.

N° 2. Shews the second order of roots, composed of its first set *aa*, of its second *bb*, of its third *cc*, of its fourth *dd*. These four sets of the second order come out

La figure entiere represente une canne à sucre dépouillée entièrement de ses feuilles.

N° 1. Presente le premier ordre de racines, elles sortent du nœud auquel est attaché le bouton qui doit produire la canne. Elles n'ont besoin pour sortir que de l'humidité contenue dans la partie médullaire du plan et de la petite fermentation qui suit le défaut de circulation ou de balancement de la sève après que les deux extrémités *y* et *z* ont été coupées.

N° 2. Presente le second ordre de racines composé de son premier plan *aa*, du second *bb*, du troisième *cc*, du quatrième *dd*; ces quatre plans du second ordre

out of the quadruple sheath *abcd*, which contained the bud. The first set of these roots cannot come out without pushing away the leaf which wraps up the whole, no more can any of the rest; I believe the part marked *r* is the neck of the plant, or the place at which it comes out of the ground.

N° 3. Shews the third order of roots, composed of its first set *ee*, of its second *ff*, of its third *gg*, and of its fourth *hh*. Each of these sets as it comes out likewise throws down one of those leaves, which in the body of the work I have called the external, or *proper* leaves of the plant. What is meant to be shewn here is rather the place than the length of them, as they are commonly between twenty and thirty inches long. *x* and *x* are the second productions or immediate growths of the stump.

These

ordre proviennent du quadruple étui de feuilles feminales *abcd*, qui enveloppoient l'embryon. Le premier plan de ces racines n'a pu sortir sans chasser la feuille enveloppe du tout; ainsi des autres. Je crois que la partie *r* forme le collet de la plante.

N° 3. Présente le troisieme ordre de cannes composé de son premier plan *ee*, du second *ff*, du troisieme *gg*, du quatrieme *hh*: chacun de ces plans fait ainsi tomber en sortant une de ces feuilles que j'appelle dans l'extrait, extérieures ou propres de la plante: je désigne ici plutôt leur place que leur longueur puisqu'elles ont ordinairement depuis vingt jusqu'à trente pouces. *x* et *x* sont les

N n 2

secondes

These second productions come from buds which adhere to sets of roots of the third order, which third order is the only one which gives productions out of the earth. The third productions, *t*, *t*, come out of the second. The number of the sets of roots of the third order is unlimited. The second is confined to four sets. The part between *i* and *k* shews the effects of a very dry season; the joints are stunted, and sometimes only two lines long, instead of an inch and a half or two inches. When the rains begin again, the joints which come out are again larger; see from *k* to *l*, where they are as big as they were between *b* and *i*.

The part from *l* to *n* is what is made use of for planting; *yz* is the plant without its leaves. It is, however, commonly put into the ground with its leaves, as
in

secondes productions propres de la souche, ces secondes productions sortent des boutons attachés aux plans de racine du troisieme ordre qui seul donne des productions hors de terre. Les troisiemes productions *t* et *t* sortent des secondes. Le nombre des plans de racine du troisieme ordre est illimité; le second ordre est borné à quatre plans. Depuis *i* jusques à *k* on voit l'effet d'un sec très vif, les nœuds sont étranglés et n'ont quelquefois que deux lignes au lieu d'un pouce et demi deux pouces. Lorsque les pluies recommencent les nœuds qui sortent sont plus longs, comme on les voit depuis *k* jusqu'à *l*, tels qu'ils étoient depuis *b* jusqu'à *i*.

La partie depuis *l* jusqu'à *n* est celle qui sert de plan; *yz* est le plan dépouillé de ses feuilles, on le met cependant en terre avec ses feuilles tel qu'il

in figure 7th. *u* is the ligament by which the bud is fastened to the plant. *oo* is the last production of the plant, and is called the arrow; it appears at first as an arrow (*oo*), and is then crowned with a pannicle *p*; it shoots in September or October, and falls in November or December. It is said, that this pannicle contains the seed of the plant.

I have sown it, but nothing has come up. When the arrow has dried, and is fallen off, the joints below it dry one after the other, when there is little or no rain; but if there happens to be much rain, the eighth and ninth, or tenth and eleventh joints from the top (see from *l* to *m*) unfold, and form a second head, which keeps up the vegetation in the lower part of the plant some time longer. If these joints were to be supported

as

qu'il est dans la figure 7. *u* est l'ombilic par lequel l'embryon est attaché au plan. *oo* est la dernière production de la canne: on l'appelle la fleche: elle paroît d'abord comme une fleche telle qu'elle est en *oo* et se couronne ensuite d'une panache *p*, elle sort en Septembre ou Octobre, et tombe en Novembre ou Decembre. On pretend que ce panache contient la graine de la plante.

J'ai semé cette prétendue graine, rien à levé. Après que la fleche est séchée et tombée, les nœuds qui la précédent se dessèchent successivement, s'il ne pleut que très peu, ou bien les huitième et neuvième, ou dixième et onzième avant dernier nœud *lm* se développent et forment une seconde tête qui entretient encore la vegetation dans le reste de la plante. Ces nœuds appuyés à mesure qu'ils

se

as they come out, they would produce canes, but of an inferior sort to the others. All canes have not arrows, and the coming out of an arrow depends on the season, and not on the age of the cane.

se développent donneroient des cannes aussi mais beaucoup moins belles que les autres ; toutes les cannes ne flechent point ; et c'est la saison et non pas l'age de la canne qui décide la sortie de la fleche.

XX. *Account of the Free Martin.*

By Mr. John Hunter, F. R. S.

Read February 25, 1779.

GENERATION, when produced from a feed, has two causes which concur towards its perfection; the one which forms the feed, the other which gives it the principle of action ^(a).

The cause which forms the feed is called the female, the other cause is called the male; but those two causes in general make only a part of a whole animal, or are

(a) It may be necessary for some of my readers to have explained to them what I mean by a feed. I do suppose, that the word feed was first applied to grain, or that which is always called feed in the vegetable; which feed is the part of that class of vegetables in which the matter of the young vegetable exists, or is formed. The principle of arrangement fitting the parts for action in this class of feed being at first not known, a false analogy between the vegetable and animal was established, viz. the secretion of the *testes* (the only known principle in the animal) was called the feed: but from the knowledge of the distinct sexes in the vegetable it is well known, that the feed is the female production in them, and that the principle of arrangement for action is from the male. The same operation and principles take place in many orders of animals, viz. the female produces a feed, in which is the matter fitted for the first arrangement of the organs of the animal, and which receives the principle of arrangement fitting them for action from the male.

rather :

rather parts superadded to an animal. Probably they were first considered in those animals where those parts were separated, or in which the female parts were wholly found in one animal and the male in the other; therefore the terms female and male have been applied to the whole animal, dividing them into two distinct sexes, and the parts which formed either the one sex or the other called either the female or the male parts of generation; but upon a further knowledge of animals, and of those parts, they were found to be united in the same animal in many of the inferior tribes, who, from possessing both parts, have got the name of hermaphrodite.

As both those parts are natural to most animals, and as the union of them in the same animal is also natural to many, and the separation of them in distinct animals, is only a circumstance making no essential difference in the parts themselves; it becomes no great effort or uncommon play in nature to unite them in those animals in which they are commonly separated.

And accordingly we find many of those orders of animals, which have them separate naturally, have them sometimes united.

From this account hermaphrodites may be divided into two kinds; the natural, and the unnatural uncommon or monstrous.

The natural belongs to the inferior and more simple order of animals, of which there are a much greater number than of the more perfect; but as animals become more complicated, have more parts, and each part is more confined to its particular use, a separation of the two necessary powers for generation have also taken place in them.

The unnatural, I believe, now and then takes place in every tribe of animals having distinct sexes, but is more common in some than in others ^(b). I fancy the human has the fewest, never having seen them in that species nor in dogs: cats we know less of; but in the horse, ass, sheep, and cattle, they are very frequent.

Though this species of hermaphrodite be a mixture of both sexes, and so possesses the parts peculiar to each in perfection, there is yet one part of each which it does not possess: I mean the part which is common to both. For as this common part is different in one sex from what it is in the other, and it is impossible for one animal to have both kinds; that which they do have must of course partake of both sexes, and consequently render the hermaphrodite imperfect *quoad hoc*.

(b) *Quære*, Is there ever in the tribe of animals, that are natural hermaphrodites, a separation of the two parts?

This one or common part is the *clitoris* in the female, and *penis* in the male; and the great difference in this part between the one sex and the other is size and perforation for the *semen*.

But those parts, which are peculiar to each sex, may be all perfectly joined in the same animal, which would come up to the idea of the truest hermaphrodite.

The hermaphrodites of this kind, which I have seen, have always appeared externally, and, at first view, to be females: and in those species of animals where only the female is preserved for breeding, as in sheep, goats, pigs, &c. they are generally saved as females.

In the horse they are very frequent: I have seen several, but never dissected any. The most perfect I have seen in this species were those in which the testicles had come down out of the *abdomen* into the place where the udder should have been (*viz.* more forward than the *scrotum*) and appeared like an udder, not so pendulous as what the *scrotum* is in the true male of such animals. There were also two nipples, which horses have no perfect form of, being blended in them with the sheath or *prepuce*, of which there was none here.

The external female parts were exactly similar to those of the perfect female; and, instead of a common-sized

sized *clitoris*, there was one about five or six inches long, which, when erect, stood almost directly backwards.

I procured a foal ass, very similar in external appearance to the above horse, and killed it, to examine the parts. It had two nipples, but the testicles were not come down as in the above; owing, perhaps, to the animal's being yet too young.

There was no *penis* passing round the *pubis* to the belly as in the perfect male ass.

The external female parts were similar to those of the she-ass. Within the entrance of the *vagina* was placed the *clitoris*, but much longer than that of a true female, being about five inches long. The *vagina* was open a little further than the opening of the *urethra* into it, and then became obliterated; from thence up to the *fundus* of the *uterus* there was no canal.

At the *fundus* of the common *uterus* it was hollow, or had a cavity in it, and then divided into two, *viz.* a right and a left, called the horns of the *uterus*, which were also pervious.

Beyond the termination of the two horns were placed the *ovaria* as in the true female, but I could not find the *fallopian* tubes.

From the broad ligaments to the edges of which the horns of the *uterus* and the *ovaria* were attached, there

passed towards each groin a part similar to the round ligaments in the female, which were continued into the rings of the *abdominal* muscles; but with this difference, that there were continued with them a *process* or *theca* of the *peritoneum*, similar to the *tunica vaginalis communis* in the male ass, and in these *thecæ* were found the testicles: but I could not observe any *vasa deferentia* passing from them.

Here then we found in the same animal the parts peculiar to each sex (although very imperfect), and that part which is common to both (but different in each) was a kind of medium of that difference.

Something similar to the above I have seen in sheep, goats, &c.; but I shall not at present trouble the Society with a description of hermaphrodites in general, as it is a very extensive subject, admitting of great variety, which would make it appear a production of chance, whereas the intention of this paper is to show a circumstance which takes place in the production of hermaphrodites in cattle, and which appearing to be an established principle in the œconomy of propagation of that species of animal, and not a production of chance, is, perhaps, peculiar to them, and, probably, the only way in which they are ever produced in this species.

It is a known fact, and, I believe, is understood to be universal, that when a cow brings forth two calves, and that one of them is a bull-calf, and the other a cow to appearance, the cow-calf is unfit for propagation; but the bull-calf becomes a very proper bull. They are known not to breed: they do not even shew the least inclination for the bull, nor does the bull ever take the least notice of them ^(c).

This cow-calf is called in this country a *free martin*; and this singularity is just as well known among the farmers as either cow or bull.

This calf has all the external marks of a cow-calf similar to what was mentioned in the unnatural hermaphrodite, *viz.* the teats and the external female parts, called by farmers the bearing.

When they are preserved it is not for propagation, but for all the purposes of an ox or spayed heifer, *viz.* to yoke with the oxen, and to fatten for the table ^(d).

They resemble in form those imperfect animals very much, *viz.* they are much larger than either the bull or the cow, and the horns grow larger, being very similar to the horns of an ox.

(c) I need hardly observe here, that if a cow has twins, and that they are both bull-calves, that they are in every respect perfect bulls; or if they are both cow-calves, that they are perfect cows.

(d) *Vide* LESLIE on Husbandry, p. 98, 99.

The

The bellow of the free martin is, similar to that of an ox, which is not at all like that of a bull; it is more of the cow, although not exactly that.

The meat is also similar to that of the ox or spayed heifer, *viz.* much finer in the fibre than either the bull or cow; and they are more susceptible of growing fat with good food. By some they are supposed to exceed the ox and heifer in delicacy of food, and bear a higher price at market.

However, it seems that this is not universal; for I was lately informed by CHARLES PALMER, esq. of Luckley in Berkshire, that there was a free martin killed in his neighbourhood, and, from the general idea of its being better meat than common, every neighbour bespoke a piece, which turned out nearly as bad as bull beef, at least worse than that of a cow. It is probable, that this might arise from this one having more the properties of the bull than the cow, as we shall see hereafter that they are sometimes more the one than the other ^(e).

Free

(e) The Romans called the bull *taurus*: they, however, talked of *tauræ* in the feminine gender. And STEPHENS observes, that it was thought the Romans meant by *tauræ*, barren cows, and called them by this name because they did not conceive any more than bulls. He also quotes a passage from COLUMELLA, lib. vi. cap. 22. “and like the *tauræ*, which occupy the place of
5 “ fertile

Free martins are said to be in sheep^(f); but from the accounts given of them, I should very much suspect that these are hermaphrodites produced in the common way, and not like those of cattle. They are often imperfect males, several of which I have seen. They are mentioned as both male and female, which is not reconcilable to the account given of the free martin.

I believe it has never been even supposed what this animal is, with all those peculiarities.

From the singularity of the animal, and the account of its production, I was almost ready to suppose the account a vulgar error; yet from the universality of its testimony it appeared to have some foundation; and therefore I made all the inquiry I could for an opportunity of seeing one, and also to examine it. Since which time I have accordingly had an opportunity of seeing three; the first of which was one belonging to JOHN ARBUTHNOT, esq. of Mitcham, which was calved in his own farm. He was so obliging as to give me an opportunity of satisfying myself. He allowed me, first, to have a drawing made of the animal while alive, which was exe-

"fertile cows, should be rejected, or sent away." He likewise quotes VARRO, *De re Rustica*, lib. ii. cap. 5, "The cow which is barren, is called *taura*." From which we may reasonably conjecture, that the Romans had not the idea of the circumstances of their production.

(f) LESLIE'S Husbandry, p. 156.

cuted.

cuted by Mr. GILPIN. When the drawing was made of Mr. ARBUTHNOT's free martin, JOHN WELLS, esq. of Bickley Farm, near Bromley in Kent, was present, and informed us, that a cow of his had calved two calves; and that one was a bull-calf, and the other a cow-calf. I desired Mr. ARBUTHNOT to speak to Mr. WELLS to keep them, or let me buy them of him; but, from his great desire for natural knowledge, he very readily preserved both, till the bull shewed all the signs of a good bull, when he sold him.

From the dissection of the three above mentioned free martins it plainly appeared, that they were all hermaphrodites differing from one another; as is also the case in hermaphrodites in other tribes.

The

*The Description of the three Free Martins.**Mr. ARBUTHNOT's Free Martin* ^(g).

The external parts were rather smaller than in the cow. The *vagina* passed on, as in the cow, to the opening of the *urethra*, and then it began to contract into a small canal, which passed on to the division of the *uterus* into the two horns, each horn passed along the edge of the broad ligament laterally towards the *ovaria*.

At the termination of those horns were placed both the *ovaria* and the testicles; both were nearly of the same size, which was about as large as a small nutmeg.

To the *ovaria* I could not find any *Fallopian* tube.

To the testicles were *vasa deferentia*, but they were imperfect. The left one did not come near the testicle; the right only came close to it, but did not terminate in a body called the *epididymis*. They were both pervious, and opened into the *vagina* near the opening of the *urethra*.

(g) This animal was about seven years old, had been often yoked with the oxen; at other times went with the cows and bull, but never shewed any desires for either the one or the other.

On the posterior surface of the bladder, or between the *uterus* and bladder, were the two bags, called *vesiculæ seminales* in the male, but much smaller than what they are in the bull: the ducts opened along with the *vasa deferentia*. This was more deserving the name of hermaphrodite than the two following; for it had a mixture of all the parts, although all were imperfect.

Mr. WRIGHT'S Free Martin, *five years old*.

The *vagina* terminated in a blind end, a little way beyond the opening of the *urethra*, beyond which the *vagina* and *uterus* were impervious. The *uterus* at its extreme part divided into two horns. At the termination of the horns were placed the testicles instead of the *ovaria*, as is the case in the female. The reasons why I call those bodies testicles are the following. First, they were more than twenty times larger than the *ovaria* of the cow, and nearly as large as the testicles of the bull, particularly as those of the ridgill, the bull whose testicles never come down. Secondly the spermatic arteries were exactly similar to those of the bull, especially of the ridgill. Thirdly, the *cremaster* muscle passed up from the
the

the rings of the abdominal muscles to the testicles, as it does in the ridgill ^(b).

There were the two bags placed behind the bladder, between it and the *uterus*. Their ducts opened into the *vagina*, a very little way beyond the opening of the *urethra*; but there was nothing similar to the *vasa deferentia*.

As the external parts had more of the cow than the bull, the *clitoris*, which may also be reckoned an external part, was also similar to that of the cow; not at all in a middle state between the *penis* of the bull and the *clitoris* of the cow, as I have described in the hermaphrodite horse. There were four teats; the glandular part of the udder was but small.

This animal cannot be said to have been a mixture of all the parts of both sexes, for the *clitoris* had nothing similar to the *penis* in the male, and was different in the cow part, in having nothing similar to the *ovaria*, nor was the *uterus* a cavity.

(b) Although I call these bodies testicles for the reason given, yet they were only imitations of such, for when cut into they had nothing of the structure of the testicle: not being similar to any thing in nature, they had more the appearance of disease. From the seeming imperfection of the animal itself, it was not to be supposed that they should be testicles, for then the animal should have partook of the bull, which it certainly did not.

Mr. WELLS's Free Martin.

This animal was never seen to shew any signs of a desire for the male, although it went constantly with one. It looked more like an heifer than what they commonly do; but as it was only between three and four years old when killed, it is very probable, that it was not sufficiently old to have taken the characters of the ox; however, this may be owing to another circumstance that will be mentioned hereafter.

The teats and udder were small compared with those of a heifer, but rather larger than in either of the former; the beginning of the *vagina* similar to that of the cow, but it soon became obliterated beyond the opening of the *urethra*, as in the last described. The *vagina* and *uterus* to external appearance was continued, although not pervious, and the uterine part divided into two horns, at the end of which were the *ovaria*.

I could not observe in this any other body which I might have supposed to be the testicle.

There was on the side of the *uterus* an interrupted *vas deferens* broken off in several places.

Behind the bladder, or between it and the *vagina*, were the bags called *vesiculæ seminales*, between which were the terminations of the two *vasa deferentia*.

The ducts of the bags and the *vasa deferentia* opened as in the former.

This could not be called an exact mixture of all the parts of both sexes, for here was no appearance of testicles.

The female parts were imperfect, and there was the addition of part of the *vasa deferentia*, and the bags called *vesiculæ seminales*.

This circumstance of having no testicles, perhaps, was the reason why it had more the external appearance of a heifer than what they commonly have, and more than either of the two former had.

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for January 1778.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
Jan.	1	8 0	34,0	35,0	29,69	N	1	Cloudy.
		2 0	35,0	35,0	29,92	N	1	Cloudy.
	2	8 0	32,5	34,5	29,95	N by W	1	Cloudy.
		2 0	36,0	35,5	29,94	NW	1	Fair.
	3	8 0	27,0	32,0	29,94	SE	1	Fair and frosty.
		2 0	36,0	34,0	29,84	SE	1	Fair and frosty.
	4	8 0	37,0	35,5	29,72	0,169 W by N	1	Cloudy.
		2 0	37,0	37,5	29,77	WNW	1	Foggy.
	5	8 0	35,5	37,0	29,80	ENE	1	Cloudy.
		2 0	36,0	37,5	29,91	NE	1	Cloudy.
	6	8 0	35,5	35,5	30,31	SE	1	Cloudy.
		2 0	37,0	37,5	30,31	NE	1	Fair.
	7	8 0	34,0	37,5	30,28	SW	1	Cloudy.
		2 0	34,0	39,0	30,26	SW	1	Cloudy.
	8	8 0	32,0	35,5	30,26	NE	1	Cloudy.
		2 0	35,0	35,5	30,27	NE	1	Fair.
	9	8 0	32,5	35,0	30,32	NE	1	Fair.
		2 0	33,0	34,5	30,30	NE	1	Fair.
	10	8 0	35,0	35,5	30,16	ENE	1	Fair.
		2 0	37,0	36,5	30,07	NW	1	Fair.
	11	8 0	32,5	35,0	29,90	NE	1	Fair.
		2 0	35,5	36,0	29,92	NE	1	Snow.
	12	8 0	33,0	35,0	29,63	ESE	1	Snow.
		2 0	38,0	37,5	29,57	ESE	1	Fine.
	13	8 0	33,5	36,0	29,43	0,222 SE	1	Snow.
		2 0	35,5	38,5	29,35	SE	1	Fair.
	14	8 0	33,5	37,0	28,87	0,095 E by S	2	Cloudy.
		2 0	37,0	39,0	28,59	ESE	1	Rain.
	15	8 0	40,0	41,0	28,68	0,139 S by E	1	Fair.
		2 0	45,0	42,5	28,76	SE	1	Rain.
	16	8 0	44,0	44,0	29,06	0,116 SSW	1	Cloudy.
		2 0	48,0	46,5	29,19	SW	1	Fair.

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for March 1878.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Mar. 1	8	0	31,5	36,0	29,17		ENE	1	Fair and frosty.
	2	0	44,0	38,0	29,17		SE	1	Fine.
2	8	0	35,5	38,0	29,21	0,326	NNE	1	Rain.
	2	0	41,0	40,0	29,18		NE	1	Fair.
3	7	0	34,5	38,0	29,27		NE by N	1	Fair.
	2	0	42,0	40,0	29,34		NE	1	Fine.
4	7	0	36,0	38,0	29,42		NNW	1	Fair.
	2	0	40,0	40,0	29,46		NW	1	Fair.
5	7	0	34,0	37,0	29,37		NE by N	1	Fair.
	2	0	40,0	39,0	29,37		NE	1	Rain.
6	7	0	34,5	38,0	29,43		WNW	1	Fair.
	2	0	41,0	40,0	29,54		NW	1	Fair.
7	7	0	36,0	39,0	29,75	0,037	NE by N	1	Rain.
	2	0	40,0	40,0	29,76		NE	1	Fair.
8	7	0	35,5	38,0	29,69	0,055	NE	1	Rain.
	2	0	40,0	39,5	29,70		ENE	1	Cloudy.
9	7	0	36,0	38,5	29,73		NE	1	Cloudy.
	2	0	39,0	41,0	29,75		NE	1	Fair.
10	7	0	37,0	38,5	29,72		NW	1	Fair.
	2	0	42,0	42,5	29,78		NW	1	Fair.
11	7	0	39,0	41,0	29,94		ENE	1	Cloudy.
	2	0	46,0	45,0	30,06		NE	1	Fair.
12	7	0	38,5	42,0	30,21		ENE	1	Fair.
	2	0	41,0	42,5	30,27		NE	1	Fair.
13	7	0	32,5	39,0	30,36		NE by E	1	Fine.
	2	0	45,0	41,0	30,43		NE	1	Fine.
14	7	0	26,0	35,0	30,38		N	1	Fair.
	2	0	41,0	39,0	30,31		NNE	1	Fine.
15	7	0	36,0	38,0	30,26		ENE	1	Fair.
	2	0	42,5	39,5	30,22		ENE	1	Fair.
16	7	0	37,5	39,0	30,19		NE	1	Fair.
	2	0	40,0	41,5	30,19		NE	1	Fair.

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for March 1778.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	36,0	40,0	30,16		SE	1	Fair.
	2	0	49,0	44,5	30,16		SE	1	Fine.
18	7	0	38,0	42,0	30,03		SW	1	Fine.
	2	0	57,0	47,0	30,01		SW	1	Fine.
19	7	0	41,5	46,0	29,97		SSW	1	Fair.
	2	0	48,0	48,5	29,88		NW	1	Fine.
20	7	0	38,0	45,0	29,59		E by N	1	Fair.
	2	0	45,0	47,0	29,53		E by N	1	Fair.
21	7	0	32,0	47,0	29,50		SW by S	1	Fair.
	2	0	57,0	49,0	29,50		SW	1	Fine.
22	7	0	49,0	50,0	29,25	0,152	SW	2	Fine.
	2	0	55,5	52,5	29,22		SW	2	Fine.
23	7	0	48,0	51,0	29,13	0,020	SW	1	Rain.
	2	0	51,5	52,5	29,02		WSW	2	Cloudy.
24	7	0	42,0	49,0	29,25	0,271	SW	1	Fine.
	2	0	47,5	50,0	29,17		SSW	2	Rain.
25	7	0	37,5	46,5	29,21	0,220	NNE	2	Cloudy.
	2	0	44,0	46,5	29,46		NNE	2	Fine.
26	7	0	34,0	40,5	30,00		NNW	1	Fine.
	2	0	46,5	43,0	30,12		SW	1	Fine.
27	7	0	36,0	41,0	30,22		SW	1	Fine.
	2	0	49,0	44,0	30,24		SW	1	Fine.
28	7	0	41,0	43,0	30,12		SSE	2	Rain.
	2	0	45,0	44,0	30,09		SW	2	Rain.
29	7	0	49,0	46,0	30,04	0,150	S by E	1	Fair.
	2	0	60,0	50,0	30,06		S by E	1	Fine.
30	7	0	51,0	50,0	29,94		SW	1	Fair.
	2	0	57,0	53,0	29,91		SW	1	Fair.
31	7	0	53,0	54,0	29,89	0,012	WSW	1	Fair.
	2	0	60,0	58,0	29,93		SW	1	Fine.

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for April 1778.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	41,0	45,0	29,64	0,026	SW	1	Fair.
	2	0	51,0	49,5	29,61		SW	1	Fine.
18	7	0	40,0	46,0	29,81	0,023	SW by S	1	Fine.
	2	0	54,0	51,0	29,95		SW	1	Fine.
19	7	0	47,0	47,5	29,94		SW by W	1	Fair.
	2	0	59,0	52,0	29,89		SW	1	Fine.
20	7	0	44,0	50,0	29,71	0,119	W by N	1	Fine.
	2	0	56,0	54,0	29,66		NNW	1	Fine.
21	7	0	41,0	47,0	29,44	0,030	NW	1	Fine.
	2	0	45,0	50,0	29,48		NW	1	Fair.
22	7	0	37,0	44,0	29,56	0,040	N by W	2	Fair.
	2	0	40,0	46,0	29,65		NW	2	Rain.
23	7	0	36,5	42,5	29,72	0,023	N by W	1	Fine.
	2	0	47,0	46,0	29,73		NW	1	Fine.
24	7	0	36,0	42,0	29,74		N by E	1	Fine.
	2	0	47,0	46,5	29,78		NE	1	Fine.
25	7	0	42,0	44,0	29,89		NE	1	Fine.
	2	0	51,5	48,5	29,91		NE	1	Fine.
26	7	0	39,5	43,5	29,73		ENE	1	Fine.
	2	0	52,5	47,0	29,65		NE	1	Fine.
27	7	0	41,0	45,0	29,45	0,025	NE	1	Rain.
	2	0	52,0	50,0	29,51		NE	1	Fair.
28	7	0	44,5	47,0	29,50	0,045	ENE	1	Fair.
	2	0	51,0	50,5	29,51		NE	1	Cloudy.
29	7	0	44,0	48,0	29,48	0,010	ENE	1	Fair.
	2	0	53,5	52,0	29,39		NE	1	Fine.
30	7	0	46,0	49,5	29,30	0,110	NE	1	Fair.
	2	0	58,0	53,0	29,32		NE	1	Fine.

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for May 1778.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
May	1	7 0	49,0	52,0	29,34	ENE	1	Cloudy.
		2 0	52,5	33,5	29,43	E by S	1	Rain.
	2	7 0	50,0	53,0	29,61	0,327 S by E	1	Fine.
		2 0	58,5	55,0	29,62	S by E	1	Rain.
	3	7 0	51,0	54,0	29,55	0,155 E by S	1	Rain.
		2 0	61,0	57,5	29,54	SE	1	Rain.
	4	7 0	53,0	56,0	29,55	0,124 SSW	1	Rain.
		2 0	65,0	62,5	29,61	SW	1	Fine.
	5	7 0	54,0	57,0	29,74	SSW	1	Fair.
		2 0	61,5	59,0	29,94	SW	1	Fine.
	6	7 0	50,0	55,5	29,98	0,038 S by E	1	Fair.
		2 0	59,0	57,5	29,95	SE	2	Fair.
	7	7 0	54,0	56,5	29,82	0,020 S by W	2	Cloudy.
		2 0	61,5	58,5	29,85	SW	1	Fine.
	8	7 0	52,5	56,0	29,94	0,012 S by E	1	Fine.
		2 0	61,0	59,0	29,94	SE	1	Cloudy.
	9	7 0	53,0	54,0	29,91	S by W	1	Fair.
		2 0	58,5	58,0	29,96	SW by W	2	Cloudy.
	10	7 0	50,0	55,0	30,20	0,046 SW	1	Fine.
		2 0	66,0	59,0	30,20	SW	1	Fine.
	11	7 0	54,5	56,0	30,10	SW by S	1	Fine.
		2 0	67,0	64,5	30,03	SW	1	Fine.
	12	7 0	56,5	58,0	29,70	SW	1	Fine.
		2 0	65,5	63,0	29,66	8W	2	Fine.
	13	7 0	53,0	59,0	29,55	0,034 S by E	1	Rain.
		2 0	64,5	62,0	29,59	SW	1	Fine.
	14	7 0	55,0	59,5	29,65	0,010 S by E	2	Fine.
		2 0	67,0	61,5	29,79	S by E	2	Fine.
	15	7 0	52,5	59,0	30,03	0,010 S by W	1	Fine.
		2 0	63,5	61,0	30,04	SW	1	Fine.
	16	7 0	55,5	59,0	29,97	S by W	1	Cloudy.
		2 0	68,0	61,5	29,99	SW	1	Fine.

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for May 1778.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
May 17	7	0	54,0	58,5	29,73	0,075	SW	2	Cloudy.
	2	0	59,0	61,5	29,67		SW	3	Cloudy.
18	7	0	51,0	58,0	29,68	0,114	SW	2	Fine.
	2	0	61,5	59,5	29,75		SW	2	Fine.
19	7	0	52,0	56,0	30,00		WSW	1	Fine.
	2	0	63,0	60,0	30,14		SW	1	Fine.
20	7	0	53,0	57,0	30,27		WSW	1	Fair.
	2	0	62,5	59,0	30,28		WSW	1	Fair.
21	7	0	55,0	58,0	30,24		NNE	1	Cloudy.
	2	0	66,0	61,0	30,20		NE	1	Fine.
22	7	0	54,5	60,0	29,96		W by S	1	Fair.
	2	0	58,5	59,5	29,92		NW by N	1	Rain.
23	7	0	50,5	57,0	30,07	0,074	NNW	1	Fair.
	2	0	60,0	58,0	30,12		NW	1	Fine.
24	7	0	51,5	56,0	30,08		SSW	1	Fine.
	2	0	62,0	57,0	30,05		SW	1	Fine.
25	7	0	51,0	54,5	29,41		NE	1	Fair.
	2	0	58,0	56,0	29,76		NE	1	Fine.
26	7	0	53,0	56,5	29,91		N by W	1	Fine.
	2	0	66,0	61,5	29,98		NE	1	Fine.
27	7	0	56,0	60,0	29,88	0,042	W by N	1	Cloudy.
	2	0	63,0	62,0	29,88		WNW	1	Fair.
28	7	0	54,0	59,0	30,03	0,052	NW	1	Fine.
	2	0	65,0	61,0	30,11		NW	1	Fine.
29	7	0	57,0	60,0	30,08		NW	1	Cloudy.
	2	0	68,5	64,0	30,03		NW	1	Fine.
30	7	0	56,0	61,5	30,07		NE	1	Fine.
	2	0	71,5	65,0	30,02		SW	1	Fair.
31	7	0	57,0	62,0	30,00		SSW	1	Fair.
	2	0	66,0	64,0	30,04		NE	1	Cloudy.

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for June 1778.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
June 17	7	0	58,5	61,0	30,12		SW	1	Fair.
	2	0	58,0	55,0	30,12		SW	1	Fair.
18	7	0	61,0	64,0	30,03	0,064	N by W	1	Fair.
	2	0	71,0	66,5	30,02		N by W	1	Fine.
19	7	0	63,5	66,5	30,09		N by E	1	Fair.
	2	0	73,5	71,0	30,13		NE	1	Fine.
20	7	0	61,0	67,0	30,18		NNE	1	Fair.
	2	0	74,0	70,5	30,22		NE	1	Fine.
21	7	0	62,0	67,0	30,29		SE	1	Fair.
	2	0	74,0	78,0	30,25		SW	1	Fine.
22	7	0	62,0	68,0	30,22		NW	1	Cloudy.
	2	0	76,5	72,0	30,21		NW	1	Fine.
23	7	0	62,5	70,0	30,22		N by W	1	Fine.
	2	0	76,0	72,5	30,17		NE	1	Fine.
24	7	0	64,5	68,5	30,08		E by N	1	Fair.
	2	0	73,5	72,5	30,02		E by N	2	Fine.
25	7	0	63,0	68,5	29,99		ENE	1	Fair.
	2	0	68,0	70,0	30,07		SW	1	Cloudy.
26	7	0	63,0	69,0	30,10	0,030	NW	1	Fine.
	2	0	78,5	72,0	30,16		NW	1	Fine.
27	7	0	66,5	70,0	30,06		SSE	1	Fine.
	2	0	82,0	75,5	30,14		SE	1	Fine.
28	7	0	63,5	70,0	29,86		SW	1	Fine.
	2	0	75,0	73,0	29,87		SW	1	Fine.
29	7	0	63,0	70,0	29,88		NNE	1	Fine.
	2	0	76,5	73,5	29,87		SW	1	Fine.
30	7	0	60,0	68,0	29,90	0,029	NE	1	Cloudy.
	2	0	77,5	73,0	29,93		NE	1	Fine.

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for July 1778.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
July	1	7 0	65,0	70,0	29,85	S by E	1	Cloudy.
		2 0	68,0	70,0	29,81	SE	1	Cloudy.
	2	7 0	63,5	67,5	30,08	W	1	Fair.
		2 0	78,0	72,5	30,12	W	1	Fine.
	3	7 0	63,0	69,0	30,01	S by W	1	Fine.
		2 0	75,0	72,5	29,98	SW	1	Fine.
	4	7 0	65,0	70,0	30,02	S by W	1	Fair.
		2 0	78,0	73,0	30,08	SW	1	Fine.
	5	7 0	70,5	72,0	29,96	ESE	1	Fine.
		2 0	82,0	78,5	29,95	SW	1	Fine.
	6	7 0	67,5	76,0	30,00	W by S	1	Fine.
		2 0	78,0	76,0	30,05	SW	1	Fine.
	7	7 0	61,5	72,5	30,04	NW by W	1	Fine.
		2 0	74,0	72,0	30,02	NW	1	Fair.
	8	7 0	62,5	70,5	30,15	NW	1	Fine.
		2 0	73,5	72,5	30,24	NW	1	Fine.
	9	7 0	63,0	66,0	30,28	NW	1	Fine.
		2 0	80,0	73,5	30,29	NW	1	Fine.
	10	7 0	66,5	69,5	30,20	W	1	Fair.
		2 0	82,5	79,0	30,20	W by N	1	Fine.
	11	7 0	66,0	75,0	30,19	NW	1	Fine.
		2 0	81,0	77,0	30,19	NW	1	Fine.
	12	7 0	68,5	75,0	30,23	NW	1	Fair.
		2 0	79,0	76,0	30,26	NW	1	Fine.
	13	7 0	72,5	77,0	30,26	ENE	1	Fine.
		2 0	86,0	81,0	30,19	SSW	1	Fine.
	14	7 0	71,5	78,0	30,11	W by S	1	Fine.
		2 0	86,0	81,0	30,08	SW	1	Fine.
	15	7 0	63,0	71,5	30,19	NE	1	Fair.
		2 0	77,5	76,5	30,23	NE	1	Fine.
	16	7 0	67,0	72,0	30,11	SE	1	Fine.
		2 0	83,5	79,5	30,06	SW	1	Fine.

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for July 1778.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.					Points.	Str.	
July 17	7	0	66,0	73,0	30,04		NNE	1	Fair.
	2	0	72,0	73,0	30,14		NE	1	Fair.
18	7	0	62,0	68,5	30,17		SE	1	Fine.
	2	0	78,0	72,0	30,12		ESE	1	Fair.
19	7	0	64,0	65,5	29,97		E by S	1	Fine and clear.
	2	0	78,0	72,0	29,94		SE	1	Clear.
20	7	0	66,5	71,0	29,57		NE	1	Clear, th and L. last night.
	2	0	69,0	73,0	29,52		S by W	1	Much th. l. and heavy rain.
21	7	0	62,5	66,0	29,48	0,990	N by E	1	Fair.
	2	0	72,0	75,0	29,54		NE	1	Clear.
22	7	0	58,5	67,5	29,76	0,186	W by S	1	Cloudy.
	2	0	67,5	66,5	29,85		W by S	1	Cloudy.
23	7	0	61,0	65,5	29,67	0,165	SSW	2	Rain, much wind last n.
	2	0	62,0	66,5	29,72		SSW	1	Rainy.
24	7	0	57,0	64,5	29,72	0,175	SSW	1	Rain last night.
	2	0	59,0	66,0	29,69		SW	1	Much rain.
25	7	0	58,5	64,5	29,62	0,872	SE	2	Rainy.
	2	0	68,5	68,0	29,57		SE	2	Rainy.
26	7	0	60,0	64,5	29,78	0,209	SW	2	Fine, much wind last n.
	2	0	68,5	67,0	29,84		SW	2	Fine, after much w. & show.
27	7	0	61,5	65,5	29,82	0,015	S by W	1	Cloudy.
	2	0	74,0	68,5	29,82		SW	1	Fine.
28	7	0	64,0	67,0	29,72	0,082	SW	2	Cloudy, wind and r. last n.
	2	0	68,5	68,0	29,75		SW	2	Showery.
29	7	0	60,0	66,0	29,79	0,046	WSW	1	Fair.
	2	0	70,0	68,5	29,99		SW	1	Fine.
30	7	0	60,5	63,0	29,88		S by E	1	Cloudy.
	2	0	72,5	78,0	29,80		SE	1	Cloudy.
31	7	0	59,0	65,0	29,77	0,209	S by E	1	Cloudy.
	2	0	65,0	65,0	29,83		SW	1	Much rain, th. and lightn.

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for August 1778.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	56,0	63,5	30,40		NE by E	1	Fine.
	2	0	72,0	69,0	30,36		NE by E	1	Fine.
18	7	0	59,5	65,0	30,24		NE	1	Fine.
	2	0	78,0	71,0	30,21		NE	1	Fine.
19	7	0	60,0	66,5	30,20		NE	1	Fine.
	2	0	79,5	72,5	30,19		SSE	1	Fine.
20	7	0	57,0	68,0	30,19		SE	1	Fair.
	2	0	78,5	71,5	30,16		SW	1	Fine.
21	7	0	62,0	69,0	30,20		NE	1	Fine.
	2	0	78,5	72,0	30,17		SE	1	Fine.
22	7	0	60,0	68,5	30,10		NE by N	1	Cloudy.
	2	0	76,5	73,0	30,09		SSE	1	Fine.
23	7	0	62,0	69,0	30,19		W by S	1	Fine.
	2	0	78,0	73,5	30,28		W by N	1	Fine.
24	7	0	60,5	68,0	30,44		WNW	1	Fine.
	2	0	78,0	73,5	30,51		NW	1	Fine.
25	7	0	65,5	71,5	30,49		NW by W	1	Fine.
	2	0	82,0	76,5	30,45		SW	1	Fine.
26	7	0	60,5	65,0	30,35		NNE	1	Fair.
	2	0	70,0	70,5	30,37		NE	1	Cloudy.
27	7	0	56,0	65,0	30,36		NE by N	1	Fair.
	2	0	65,0	65,5	30,33		N by E	2	Cloudy.
28	7	0	55,0	61,5	30,40		NE	2	Fine.
	2	0	69,0	65,0	30,43		NE	2	Fine.
29	7	0	52,0	60,5	30,44		N by W	1	Fine.
	2	0	66,0	63,5	30,38		NW	1	Fine.
30	7	0	55,0	61,0	30,08		WSW	1	Fine.
	2	0	68,0	64,5	29,98		W by N	1	Cloudy.
31	7	0	47,0	57,5	30,03	0,044	NW	1	Fine.
	2	0	61,0	59,5	30,14		NW	2	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Sept.	1	7 0	44.5	55.0	30.13		SW	1	Fair.
		2 0	64.0	59.0	30.09		SW	1	Fine.
	2	7 0	55.5	58.5	29.90		WNW	1	Rain.
		2 0	63 0	61.5	29.94		NW	1	Cloudy.
	3	7 0	55.5	60.0	29.88	0.027	N by W	1	Cloudy.
		2 0	67.0	63.5	29.86		NW	1	Cloudy.
	4	7 0	49.0	60.0	30.01		NE	1	Fine.
		2 0	65.0	62.0	29.98		NW	1	Fine.
	5	7 0	50.0	59.0	30.00	0.015	NE by E	1	Fine.
		2 0	63.5	61.5	30.01		NW	1	Fine.
	6	7 0	47.0	53.5	30.01		NE	1	Fine.
		2 0	64.5	59.5	29.99		NE	1	Cloudy.
	7	7 0	56.0	58.0	29.87		NE	1	Cloudy.
		2 0	61.0	61.5	29.85		NE	1	Rainy.
	8	7 0	53.5	59.5	29.95	0.024	NNE	1	Fine.
		2 0	68.0	63.5	30.03		NE	1	Fine.
	9	7 0	52.0	59.0	30.09		NNE	1	Cloudy.
		2 0	64.5	62.0	30.05		ENE	1	Fine.
	10	7 0	52.5	59.0	29.98		NE	1	Cloudy.
		2 0	65.0	62.5	29.93		SE	1	Fine.
	11	7 0	55.5	59.5	29.54		E	1	Cloudy.
		2 0	54.0	60.5	29.48		NE	1	Rainy.
	12	7 0	53.0	58.0	29.48	0.375	SW	1	Fair.
		2 0	61.5	61.0	29.50		W by N	2	Fine.
	13	7 0	47.0	57.0	29.68	0.015	W by N	1	Fine.
		2 0	63.5	60.5	29.78		NW	1	Fine.
	14	7 0	45.5	56.0	30.05		NE	1	Fair.
		2 0	63.5	59.0	30.15		NE	1	Fine.
	15	7 0	46.5	54.5	30.23		NE	1	Fine.
		2 0	66.0	59.5	30.20		SE	1	Fine.
	16	7 0	48.0	56.0	29.96		S by E	1	Fine.
		2 0	66.5	62.0	29.80		SE	1	Cloudy.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	46,0	56,0	29,86	0,146	SSW	1	Fine.
	2	0	60,0	59,0	29,89		SW	1	Fine.
18	7	0	43,5	53,5	30,04		SW by S	1	Fine.
	2	0	63,0	57,5	30,09		WNW	1	Fine.
19	7	0	51,0	56,5	30,27		SW	1	Fine.
	2	0	66,5	60,0	30,30		WSW	1	Fine.
20	7	0	50,5	56,5	30,42		ENE	1	Fine.
	2	0	68,0	62,0	30,40		S	1	Fine.
21	7	0	49,5	57,0	30,42		NE	1	Fair.
	2	0	67,5	62,0	30,42		NE by E	1	Fine.
22	7	0	53,0	58,0	30,38		NE by N	1	Cloudy.
	2	0	69,0	67,5	30,39		NE	1	Fine.
23	7	0	58,5	62,5	30,26		E by N	1	Cloudy.
	2	0	71,0	67,5	30,21		NE	1	Fine.
24	7	0	52,0	62,5	29,99		SSW	1	Fair.
	2	0	67,5	65,0	29,99		NNW	1	Fine.
25	7	0	48,0	58,5	29,98		NNE	1	Fair.
	2	0	69,5	60,5	29,94		NE	1	Fine.
26	7	0	47,0	56,0	29,62		NE	1	Cloudy.
	2	0	58,5	57,5	29,45		SSE	1	Cloudy.
27	7	0	49,0	56,0	29,15	0,100	NNE	1	Cloudy.
	2	0	49,5	56,0	29,27		N by W	2	Rainy.
28	7	0	37,0	49,0	29,94	0,033	W by S	1	Fine.
	2	0	51,0	50,5	30,07		W by S	1	Fine.
29	7	0	36,5	47,0	30,27		S by W	1	Fair.
	2	0	53,5	50,0	30,30		SW	1	Fine.
30	7	0	45,5	49,0	30,03		SW	1	Fine.
	2	0	57,0	52,0	29,86		SSW	2	Fine.

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		Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
		H. M.			Inches.	Inch.	Points	Str.	
Oct.]	1	7 0	48,0	52,0	29,47	0,147	SSW	1	Fine.
		2 0	56,5	55,5	29,42		SW	1	Showery.
	2	7 0	47,0	52,5	29,37	0,132	WSW	1	Fine.
		2 0	54,5	54,5	29,41		SW	1	Showery.
	3	7 0	41,0	50,5	29,34	0,040	SW	1	Fair.
		2 0	54,5	53,0	29,36		NW	1	Fine.
	4	7 0	41,0	50,0	29,50		SSW	1	Fair.
		2 0	54,0	52,5	29,58		NW	1	Cloudy and showery.
	5	7 0	42,0	50,0	29,66		N by W	1	Fair.
		2 0	57,0	53,5	29,69		NW	1	Fair.
	6	7 0	44,0	50,5	29,60		NE by E	1	Cloudy.
		2 0	54,5	52,5	29,54		NE	1	Cloudy.
	7	7 0	55,0	54,0	29,30	0,543	ENE	1	Rainy.
		2 0	64,5	68,5	29,27		ENE	1	Cloudy.
	8	7 0	54,0	58,0	29,30	0,560	SSE	1	Fine.
		2 0	61,5	67,0	29,29		NE	1	Fine.
	9	7 0	48,0	56,5	29,38	0,052	NE	1	Cloudy.
		2 0	59,0	59,0	29,38		NE	1	Fine.
	10	7 0	52,0	57,0	29,40	0,207	NW by N	1	Cloudy.
		2 0	51,0	56,5	29,60		NE	1	Rainy.
	11	7 0	38,5	50,5	29,75	0,010	WNW	1	Fine.
		2 0	51,0	51,5	29,76		NW	1	Fine.
	12	7 0	34,0	46,0	29,91		W by N	1	Fair.
		2 0	51,0	48,5	29,96		W by N	1	Fine.
	13	7 0	40,0	46,0	29,89		NE	1	Fair.
		2 0	53,0	49,0	29,85		NE	1	Fair.
	14	7 0	35,5	45,5	29,72		NW	1	Fine.
		2 0	51,0	49,0	29,70		NW	2	Fine.
	15	7 0	38,5	45,0	29,64		NE by N	1	Fine.
		2 0	61,0	59,5	29,79		N	1	Cloudy.
	16	7 0	36,5	40,5	29,86		N	1	Fine.
		2 0	49,0	45,5	29,92		NE	1	Fair.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	32,0	41,0	30,00		NNE	1	Fine.
	2	0	47,0	44,0	29,98		N by E	1	Fine.
18	7	0	31,5	40,0	29,98		S by E	1	Fair and frosty.
	2	0	50,0	43,5	30,00		SE	1	Fine.
19	7	0	33,5	40,5	29,90		NE by N	1	Fine.
	2	0	47,0	43,0	29,81		NE	1	Fine.
20	7	0	40,0	41,5	29,44		NE	1	Fair.
	2	0	49,0	45,0	29,44		NE	1	Cloudy.
21	7	0	39,5	43,5	29,52	0,025	WSW	1	Foggy.
	2	0	55,0	49,0	29,58		SW	1	Fine.
22	7	0	41,5	47,0	29,64	0,028	SW	1	Fine.
	2	0	53,0	49,0	29,75		SW	1	Fine.
23	7	0	54,5	50,0	29,31	0,320	S by E	2	Rainy.
	2	0	59,0	54,0	29,21		SW	2	Rainy.
24	7	0	54,5	54,5	29,18	0,465	NW	2	Rainy.
	2	0	61,5	57,0	29,17		NW	2	Showery.
25	7	0	52,5	56,5	29,20	0,133	SW by W	2	Cloudy.
	2	0	45,0	55,0	29,31		NE	1	Rainy.
26	7	0	33,5	47,0	29,79	0,037	NW by N	1	Fine.
	2	0	45,0	47,5	29,89		NW	1	Fine.
27	7	0	34,0	42,0	29,90		NE by N	1	Fair.
	2	0	47,0	45,0	29,89		NW	1	Fine.
28	7	0	45,0	44,5	29,74		ESE	1	Cloudy.
	2	0	54,0	51,0	29,72		ESE	1	Cloudy.
29	7	0	52,0	49,0	29,60	0,111	SSE	1	Cloudy.
	2	0	56,5	53,0	29,79		SE	1	Cloudy.
30	7	0	49,5	52,0	30,02	0,060	SW by W	1	Fair.
	2	0	55,0	53,0	30,16		SW	2	Fair.
31	7	0	52,0	52,5	29,84	0,033	NW	2	Cloudy.
	2	0	57,5	55,0	29,69		SW	3	Cloudy.

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	Time		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	49,5	50,5	29,59	0,249	SSW	1	Fair.
	2	0	52,0	51,5	29,58		SW	1	Fair.
18	8	0	44,0	49,5	29,61		WSW	1	Fair.
	2	0	47,0	48,0	29,66		SW	1	Fine.
19	8	0	45,5	47,5	29,87		N by W	1	Fair.
	2	0	47,0	47,5	29,99		NW	1	Cloudy.
20	8	0	45,5	48,0	29,97		SE by E	1	Cloudy.
	2	0	48,0	48,5	29,90		SE	1	Rainy.
21	8	0	46,0	47,5	30,00	0,234	S by W	1	Cloudy.
	2	0	50,0	49,0	30,01		SW	1	Rainy.
22	8	0	49,5	49,0	29,95		SW	1	Cloudy.
	2	0	51,0	52,0	29,90		SW	1	Cloudy.
23	8	0	55,0	53,0	29,63	0,168	SW	2	Cloudy.
	2	0	57,0	52,5	29,64		SW	2	Cloudy.
24	8	0	56,5	56,0	29,53	0,263	SW	2	Fair.
	2	0	56,0	57,5	29,45		SW	3	Rainy.
25	8	0	45,0	53,0	29,73	0,261	SW	2	Fine.
	2	0	49,0	52,0	29,96		SW	2	Fine.
26	8	0	41,0	48,0	30,10		WSW	1	Fine.
	2	0	48,5	50,0	29,84		SW	1	Fine.
27	8	0	50,0	51,0	29,26	0,318	SW	3	Rain and windy.
	2	0	45,0	49,5	29,35		SW	1	Fair.
28	8	0	41,5	48,5	29,85	0,025	SW	1	Fair.
	2	0	50,0	50,0	29,95		SW	1	Fine.
29	8	0	43,5	47,0	29,98		SE by S	1	Fair.
	2	0	50,0	38,0	29,88		NE	1	Fine.
30	8	0	42,0	47,5	29,77	0,073	NW	1	Fair.
	2	0	49,0	50,0	29,86		NW	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	32,0	42,0	30,12		SW	1	Fair and frosty.
	2	0	40,0	41,0	30,02		SW	1	Fine.
2	8	0	36,0	40,0	29,54		SE	1	Fair.
	2	0	40,0	41,5	29,42		SW	1	Cloudy.
3	8	0	40,5	41,5	29,19	0,125	SE	1	Fair.
	2	0	45,0	46,0	29,16		SE	1	Cloudy.
4	8	0	43,0	44,5	28,55	0,495	SSE	2	Rain and much wind last n.
	2	0	44,0	43,0	28,79		SE	2	Rainy.
5	8	0	37,0	43,0	29,00	0,038	S	1	Fair.
	2	0	42,0	43,5	28,91		NNE	1	Rainy.
6	8	0	35,0	40,5	29,19	0,130	SW	1	Fair.
	2	0	40,5	44,5	29,37		SW	1	Fine.
7	8	0	50,5	44,5	29,03	0,206	SW by S	2	Fair.
	2	0	47,0	46,0	29,32		SW	2	Fair.
8	8	0	50,5	46,5	29,40	0,130	SSW	3	Rain and much wind last n.
	2	0	55,0	58,5	29,45		SSW	3	Rainy.
9	8	0	48,5	50,5	29,79	0,187	SW	1	Fair.
	2	0	56,0	53,0	29,88		SW	1	Fair.
10	8	0	49,5	52,0	29,67	0,114	S by W	1	Cloudy.
	2	0	52,0	53,0	29,64		SW	1	Cloudy.
11	8	0	51,5	52,0	29,63	0,135	SW	2	Cloudy.
	2	0	54,5	53,5	29,58		SW	2	Cloudy.
12	8	0	53,0	54,0	29,48	0,036	SW by S	2	Cloudy.
	2	0	53,0	54,5	29,38		SW	2	Fine.
13	8	0	47,5	52,5	29,13	0,051	SW	1	Fair.
	2	0	48,0	52,5	29,21		SW	1	Rain.
14	8	0	42,0	48,5	29,44	0,350	SW by S	1	Fair.
	2	0	48,0	50,0	29,39		SE	1	Rainy and cloudy.
15	8	0	41,0	46,0	29,66	0,096	W by S	2	Fair.
	2	0	44,0	46,5	29,88		SW	1	Fine.
16	8	0	34,5	41,0	30,15		SW	1	Fair.
	2	0	42,0	43,0	30,21		SW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Dec. 17	8	0	43,0	43,5	30,25		WSW	1	Cloudy.
	2	0	47,5	44,5	30,33		SW	1	Fine.
18	8	0	48,0	47,0	30,33		SW	1	Cloudy.
	2	0	52,0	50,0	30,36		SW	1	Cloudy.
19	8	0	49,5	51,0	30,19		SW	2	Cloudy.
	2	0	52,5	51,5	30,17		SW	1	Cloudy.
20	8	0	38,5	47,0	30,34	0,051	W by S	1	Fair.
	2	0	45,0	47,0	30,32		SW	1	Fine.
21	8	0	45,0	47,0	30,37		SW	1	Foggy.
	2	0	47,0	49,0	30,44		W	1	Foggy and cloudy.
22	8	0	44,5	47,5	30,38		WSW	1	Cloudy.
	2	0	45,0	48,5	30,35		WSW	1	Cloudy.
23	8	0	43,5	48,0	30,31	0,010	WSW	1	Rainy.
	2	0	45,0	50,0	30,36		NW	1	Cloudy and rain.
24	8	0	44,5	47,5	30,54	0,032	N by E	1	Cloudy and rainy.
	2	0	48,0	48,0	30,64		NE	1	Cloudy and rainy.
25	8	0	44,5	48,5	30,71	0,033	ESE	1	Foggy.
	2	0	46,0	49,0	30,75		ESE	1	Foggy and Cloudy.
26	8	0	34,5	44,5	30,77		SSW	1	Clear.
	2	0	40,0	45,0	30,81		SSW	1	Clear and fine.
27	8	0	33,0	40,0	30,73		SSW	1	Foggy.
	2	0	38,5	41,0	30,64		SSE	1	Cloudy.
28	8	0	39,0	41,5	30,16		SW	1	Fair.
	2	0	45,0	44,5	30,09		SW	1	Cloudy.
29	8	0	44,0	43,0	29,76	0,060	WSW	2	Rain and much wind left n.
	2	0	50,0	47,5	29,58		NW	2	Rain and cloudy.
30	8	0	35,5	42,0	29,89	0,040	W	1	Cloudy.
	2	0	42,0	43,0	29,87		SW	1	Fine.
31	8	0	48,5	45,5	29,60	0,125	W by S	2	Cloudy.
	2	0	50,0	46,5	29,18		SW	3	Cloudy.

M E T E.

1778.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January	48,0	22,0	37,1	48,5	30,0	38,9	30,22	28,59	29,88	2,111
February	49,0	25,5	38,2	48,0	32,5	39,6	30,35	29,15	29,82	0,527
March	60,0	31,5	42,3	58,0	35,0	43,4	30,43	29,02	29,78	1,243
April	71,5	34,5	50,0	63,0	42,0	51,3	30,24	29,30	29,76	0,734
May	71,5	49,0	58,5	65,0	52,0	58,4	30,28	29,34	29,89	1,133
June	82,0	51,0	65,0	78,0	55,0	66,3	30,36	29,64	30,05	0,957
July	86,0	58,5	70,3	81,0	63,0	71,0	30,29	29,48	29,98	3,053
August	82,0	47,0	67,0	76,5	57,5	68,0	30,51	29,59	30,31	1,039
September	71,0	36,5	56,1	67,5	47,0	58,4	30,42	29,15	29,77	0,735
October	64,5	31,5	48,3	68,5	40,0	50,3	30,16	29,17	29,63	2,903
November	57,0	32,5	47,0	57,5	38,0	45,1	30,27	28,95	29,68	3,897
December	56,0	32,0	44,7	58,5	40,0	46,7	30,81	28,55	29,85	2,440
Whole Year			52,0			53,1			29,86	20,772

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V A R I A-

VARIATION NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Daily Means.
June 29	° ' -	° ' -	° ' -	22 22	22 23
30	22 7	22 22	22 25	22 18	22 18
July 1	22 7	22 28	22 27	22 13	22 19
2	22 3	22 24	22 20	22 7	22 13
3	22 7	22 27	22 30	22 18	22 20
4	22 10	22 24	22 23	22 14	22 18
5	22 8	22 25	22 27	22 24	22 21
6	22 12	22 26	22 28	22 21	22 22
7	22 9	22 29	22 43	22 14	22 24
8	22 18	22 19	22 25	22 20	22 21
9	22 10	22 26	22 28	22 28	22 23
10	22 9	22 27	22 28	22 27	22 23
11	22 9	22 26	22 24	22 21	22 20
12	22 8	22 28	22 36	22 2	22 18
13	22 10	22 30	22 30	- -	22 17
Means	22 9	22 26	22 28	22 18	
Mean of all $22^{\circ} 20\frac{1}{4}$.					

It was found by the method described in Phil. Transf.
 vol. LXVI. p. 387. that the variation shewn by the in-
 Vol. LXIX. T t frument

strument is $9\frac{1}{4}$ too great, so that the mean variation is really only $22^{\circ} 11'$; and, therefore, as the variation at the Society's House seems to be $15\frac{1}{2}$ greater than it ought to be, by reason of the magnetism of the building (*vide* the same place) the true variation seems to be $21^{\circ} 55\frac{1}{2}$.

The reason why the instrument now shews the variation $9\frac{1}{4}$ too great, whereas before the error was insensible is, that a small alteration has been made in the agate cap of the needle since the last year's observations, as it was thought not to have been fastened before in a sufficiently steady manner.

DIPPING

DIPPING NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 P.M.	Mean.	
June 29	° ' -	° ' -	° ' -	73 35	° ' -	
30	73 20	73 20	73 15	73 10	73 16	West mark down.
July 1	73 15	73 20	73 10	73 10		
2	73 15	73 15	73 10	73 20		
3	73 10	71 50	71 45	71 50		East.
4	71 50	71 50	71 50	71 50	71 47	
5	71 50	71 45	71 45	71 40		
6	71 50	71 40	71 40	71 25		West mark uppermost.
7	71 20	71 25	71 10	71 20	71 25	
8	71 25	71 25	71 25	71 30		
9	71 25	71 50	71 25	71 30		East
10	71 15	73 15	73 10	73 10		
11	73 20	73 30	73 10	73 40	73 16	
12	73 10	73 10	73 0	73 10		
13	73 20	73 15	73 25	- -		

THE END OF VOL. LXIX. PART I.

VOL. LXVIII. PART II.

789. 12. *for* ARTEDE *read* ARTEDI

13. 19. *for* was read were
15. 5. *for ericæ forme* read *ericæ-forme*
18. 7. *for erica*, formed, read *erica-formed*
21. 13. *for* produces read produce
32. 8. *for* it read its
79. 15. *for supposito pro* supposito
118. 7. *for* at the sea read at sea
123. 1. *for* ○ read ○
132. 3. *for* obferyation read observations
149. 8. *for* kind *meatus* read kind of *meatus*

PHILOSOPHICAL
TRANSACTIONS.

PART II.

VOL. LXIX.

Un'

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

VOL. LXIX. For the Year 1779.

PART II.

LONDON,

PRINTED BY J. NICHOLS, SUCCESSOR TO MR. BOWYER;
FOR LOCKYER DAVIS, PRINTER TO THE ROYAL SOCIETY.

MDCCLXXX.

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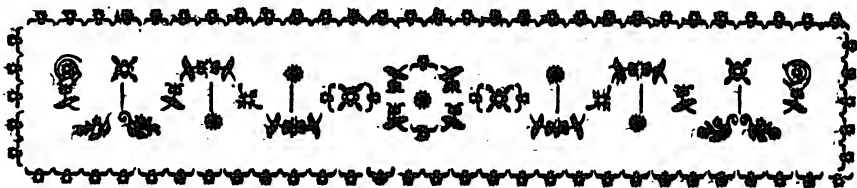
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PHILOSOPHICAL
TRANSACTIONS.

XXII. *Account of the Manner in which the Russians treat Persons affected by the Fumes of burning Charcoal, and other Effluvia of the same Nature. In a Letter from Matthew Guthrie, M. D. to Joseph Priestley, LL.D. F. R. S.*

Read March 4, 1779.

DEAR SIR,

St. Petersburg,
October 12, 1778.

I SHALL endeavour to recollect, according to your desire, the particulars of that part of my former letter which related to the mode of recovering people in Russia, who are apparently deprived of life by the prin-

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X x

ciple

ciple emitted from burning charcoal, or by the incrustation formed upon the insides of the boors huts when it thaws.

People of condition in this country have double windows to their houses in winter; but the commoner sort have only single ones, which is the reason that, during a severe frost, there is an incrustation formed upon the insides of the glass windows. This seems to be composed of condensed breath, perspiration, &c. as a number of people live and sleep in the same small room, especially in great cities. This excrementitious crust is farther impregnated with the phlogiston of candles, and of the oven with which the chamber is heated.

When a thaw succeeds a hard frost of long duration, and this plate of ice is converted into water, there is a principle set loose, which produces all the terrible effects upon the human body which the principle emitted from charcoal is so well known to do in this country, where people every day suffer from it. However, the Russians constantly lay the blame upon the oven, when they are affected by the thawing of the crust, as the effects are perfectly similar, and they cannot bring themselves to believe, that the dissolving of so small a portion of ice can be attended with any bad consequence, when they daily melt larger masses without danger: yet the oven
does.

does not at all account for the complaints brought on at this period; for, upon examination, they generally find every thing right there, and still the *ugar*, or hurtful vapour, remaining in the room.

As the effects of both are similar, as I have said above, and likewise the mode of recovery, I shall only give you an account of the operation of the principle emitted by burning charcoal, and of the method of bringing those people to life who have been *suffocated* by it (as I think it is erroneously termed); this will supersede the necessity of giving the history of both, or rather it will be giving both at the same time.

Russian houses are heated by the means of ovens; and the manner of heating them is as follows. A number of billets of wood are placed in the *peeck* or stove, and allowed to burn till they fall in a mass of bright red cinders; then the vent above is shut up, and likewise the door of the *peeck* which opens into the room, in order to concentrate the heat; this makes the tiles of which the *peeck* is composed as hot as you desire, and sufficiently warms the apartment; but sometimes a servant is so negligent as to shut up the *peeck* or oven before the wood is sufficiently burnt, for the red cinders should be turned over from time to time to see that no bit of wood remains of a blackish colour, but that the whole mass is

X x 2

of

of a uniform glare (as if almost transparent) before the openings are shut, else the *ugar* or vapour is sure to succeed to mismanagement of this sort, and its effects are as follows.

If a person lays himself down to sleep in the room exposed to the influence of this vapour, he falls into so sound a sleep that it is difficult to awake him, but he feels (or is sensible of) nothing. There is no spasm excited in the *trachea arteria* or lungs to rouse him, nor does the breathing, by all accounts, seem to be particularly affected: in short, there is no one symptom of suffocation; but towards the end of the catastrophe, a sort of groaning is heard by people in the next room, which brings them sometimes to the relief of the sufferer. If a person only sits down in the room, without intention to sleep, he is, after some time, seized with a drowsiness and inclination to vomit. However, this last symptom seldom affects a Russian, it is chiefly foreigners who are awakened to their dangers by a *nausea*; but the natives, in common with strangers, perceive a dull pain in their heads, and if they do not remove directly, which they are often too sleepy to do, are soon deprived of their senses and power of motion, inasmuch, that if no person fortunately discovers them within an hour after this worst stage, they are irrecoverably lost; for the Russians say, that they

do not succeed in restoring to life those who have lain more than an hour in a state of insensibility.

The recovery is always attempted, and often effected, in this manner. They carry the patient immediately out of doors, and lay him upon the snow, with nothing on him but a shirt and linen drawers. His stomach and temples are then well rubbed with snow, and cold water, or milk is poured down his throat. This friction is continued with fresh snow until the livid hue, which the body had when brought out, is changed to its natural colour, and life renewed; then they cure the violent head-ach which remains by binding on the forehead a cataplasm of black rye bread, and vinegar.

In this manner the unfortunate man is perfectly restored, without blowing up the lungs, as is necessary in the case of drowned persons; on the contrary, they begin to play of themselves so soon as the furcharge of phlogiston makes its escape from the body.

It is well worthy of observation, how diametrically opposite the modes are of restoring to life, those who are deprived of it by water, and those who have lost it by the fumes of charcoal: the one consisting in the internal and external application of heat, and the other in that of cold. It may be alledged, that the stimulus of the cold produces heat, and the fact seems to be confirmed by the Russian method of restoring circulation in a frozen limb

limb by means of friction with snow. But what is singular in the case of people apparently deprived of life in the manner treated of is, that the body is much warmer when brought out of the room than at the instant life is restored, and that they awake cold and shivering. The colour of the body is also changed from a livid red to its natural complexion, which, together with some other circumstances, would almost lead one to suspect, that they are restored to life by the snow and cold water somehow or other freeing them from the load of phlogiston with which the system seems to be replete; for although the first application of cold water to the human body produces heat, yet, if often repeated in a very cold atmosphere, it then cools instead of continuing to heat, just as the cold bath does when a person remains too long in it.

In short, I think it is altogether a curious subject, whether you take into consideration the mode of action of the principle emitted by burning charcoal, and our phlogificated crust; or the operation of the snow and cold water. However, I shall by no means take upon me to decide, whether the dangerous symptoms related above are produced by the air in the room being so saturated with phlogiston as to be unable to take up the proper quantity from the lungs, which occasions a surcharge in the system, according to your theory, or whether

ther so subtle a fluid may somehow find its way into the circulation, and thereby arrest the vital powers; nor shall I determine whether the livid hue of the body when brought out is changed into a paler colour by the atmosphere somehow or other absorbing and freeing the blood from the colouring principle, as you have shewn to be the case with blood out of the body: these are curious inquiries that I shall leave to your investigation. I have only endeavoured to collect facts from a number of natives who have met with this accident themselves, or have assisted in restoring others to life. It is so common a case here that it is perfectly familiar to them, and they never call in medical assistance.

I am, &c.

XXIII. *An Account of an Apparatus applied to the equatorial Instrument for correcting the Errors arising from the Refraction in Altitude. By Mr. Peter Dollond, Optician; communicated by the Astronomer Royal.*

Read March 4, 1779.

THE refraction of the atmosphere occasions the stars or planets to appear higher above the horizon than they really are; therefore, a correction for this refraction should be made in a vertical direction to the horizon.

The equatorial instrument is so constructed, that the correction cannot be made by the arches or circles which compose it when the star, &c. is in any other vertical arch except that of the meridian, because the declination arch is never in a vertical position but when the telescope is in the plane of the meridian.

To correct this error, a method of moving the eye-tube which contains the wires of the telescope in a vertical direction to the horizon has been practised; but as the eye-tube is obliged to be turned round in order to
move

move it in that direction, in the different oblique positions of the instrument, the wires are thereby put out of their proper situation in every other position of the instrument, except when it is in the plane of the meridian; for the equatorial wire should always be parallel to the equator, that the star in passing over the field of the telescope may move along with it, otherwise one cannot judge whether the telescope be set to the proper declination, except at the instant the star is brought to the intersection of the wires, which is only a momentary observation.

The method I have now put in practice for correcting the refraction of the atmosphere is by applying two lenses before the object-glass of the telescope; one of them convex, and the other concave; both ground on spheres of the same radius, which in those I have made is thirty feet. The convex lens is round, of the same diameter as the object-glass of the telescope, and fixed into a brass frame or apparatus, which fits on to the end of the telescope. The concave lens is of the same width, but nearly two inches longer than it is wide, and is fixed in an oblong frame, which is made to slide on the frame the other lens is fixed into, and close to it. These two lenses being wrought on spheres of the same radius, the refraction of the one will be exactly destroyed by that of

the other, and the focal length of the object-glass will not be altered by their being applied before it; and if the centers of these two lenses coincide with each other, and also with that of the object-glass, the image of any object formed in the telescope will not be moved or suffer any change in its position. But if one of the lenses be moved on the other, in the direction of a vertical arch, so as to separate its center from that of the other lens, it will occasion a refraction, and the image will change its altitude in the telescope. The quantity of the refraction will be always in proportion to the motion of the lens, so that by a scale of equal parts applied to the brass frame, the lens may be set to occasion a refraction equal to the refraction of the atmosphere in any altitude. If the concave lens be moved downwards, that is, towards the horizon, its refraction will then be in a contrary direction to that of the atmosphere, and the star will appear in the telescope as if no refraction had taken place.

There is a small circular spirit level fixed on one side the apparatus, which serves to set it in such a position, that the centers of the two lenses may be in the plane of a vertical arch. This level is also used for adjusting a small quadrant, which is fixed to it, and divided into degrees, to shew the elevation of the telescope when directed to the star; then the quantity of refraction answering

fwering to that altitude may be found by the common tables, and the concave lens set accordingly, by means of the scale at the side, which is divided into half minutes, and, if required, by using a nonius, may be divided into seconds.

It must be observed, that when a star or planet is but a few degrees above the horizon, the refraction of the atmosphere occasions it to be considerably coloured. The refraction of the lens acting in a contrary direction would exactly correct that colour, if the dissipation of the rays of light were the same in glass as in air; but as it is greater in glass than in air, the colours occasioned by the refraction of the atmosphere will be rather more than corrected by those occasioned by the refraction of the lens.

A drawing of the refraction apparatus is added, which may serve to give a more clear idea of it. See plate IV.

EXPLANATION OF THE PLATE.

AA, The circular brass tube, which fits on to the end of the telescope.

BB, The oblong concave lens in its frame, which slides over the fixed convex lens.

Y y 2

c, The

- c, The circular spirit level, which shews when the oblong lens is in a vertical arch.
- d, The quadrant to which the spirit level is fixed, for shewing the angular elevation of the telescope.
- e, The milled head fixed to a pinion, by which the whole apparatus is turned round on the end of the
in a vertical

to the angu-
of these two
the middle

minutes and
by the con-

IV. p. 336.

XXIV. *Experiments and Observations on the inflammable Air breathed by various Animals. By the Abbé Fontana, Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of Tuscany; communicated by John Paradife, Esq. F. R. S.*

Read March 11, 1779.

PHILOSOPHERS believed, till lately, that inflammable air had the power of killing animals who breathed it. Dr. PRIESTLEY, to whom we are much indebted for many discoveries and observations relative to inflammable air, made in consequence of Mr. CAVENDISH's excellent paper on that subject, assures us, that inflammable air causes the death of animals as readily as fixed air, and that animals die convulsed in it. The doctor adds, that water absorbed about one quarter of the inflammable air shaken in it, after which a mouse lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouse was still inflammable, though not so much as before.

Mr.

Mr. SHEELE, who has made various important observations in chemistry, on the contrary asserts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates some experiments to which it seems that nothing can be opposed, and they appear to contradict Dr. PRIESTLEY's observations. Mr. SHEELE has breathed inflammable air contained in a bladder, without receiving any hurt.

Seeing then that the experiments of these celebrated persons contradicted each other, I began to suspect that they might possibly be all true; and that their so contradictory effects might be owing to some circumstance not yet attended to.

In order to follow some method in my researches about a point so delicate, and which so nearly interests human life, I first of all thought of assuring myself, whether or no animals could breathe inflammable air with impunity, when the receivers that contained it were immersed in quicksilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quicksilver, in which the air entered pretty free from moisture. I then introduced various birds into those tubes, and observed that they died in a few minutes
I time,

time, but without any apparent sign of convulsions. These experiments, having been often repeated, were constantly attended with the same event.

Being assured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to pass through quicksilver, was fatal to animals; I next wished to observe, whether it retained the same properties when it had passed through water; in which case the volatile sulphurous acid, or other vapour, is absorbed by the water; but, on trying the experiments, I found that the birds died under these circumstances as under the others (though not quite so soon) shewing likewise some signs of convulsion. I introduced some of this same air that had passed through water into a glass tube full of quicksilver, by a method which makes the air lose all its moisture. The birds died in it in the same manner as when the experiment was tried upon water. In all these cases the air after the animals had died in it was still inflammable, nor did its exploding properties seem to have been at all diminished.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been shaken in water for a minute's time, or something longer. By shaking it a long time, it becomes in some measure respirable; but then it is decomposed in a great measure,

measure, and becomes of another kind, although it still preserves the properties of being inflammable, but in a smaller degree,

Not only birds but also quadrupeds die in inflammable air (though not so soon) and shew some signs of being convulsed.

It seems very strange, that Mr. SHEELE could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very short time. Admitting his experiments to be true, there remains nothing to be said, but that the inflammable air in which animals die does not occasion death because it is conveyed to the lungs, but because it affects some other organs of the animal body exposed to that air, and necessary to animal life. It is not impossible to occasion death by affecting the very sensible nerves of the nose; it being well known, that various liquors, as very concentrated volatile alkaly, &c. if they are inspired through the nose, immediately affect the senses, and occasion death if they continue to act upon the *pituitary* membrane.

In order, therefore, to try whether inflammable air killed, only because it was inspired through the nose, I stopped very accurately the noses of various birds with soft wax, and in this manner I introduced them into
receivers

receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few seconds, that is, just as they did when their noses were unstopped. Quadrupeds were tried after the same manner, and the event was the same.

Having in this manner exploded this new hypothesis, there remained one more, which seemed to suggest a probable reason (since some reason there must be) for Mr. SHEELE's experiments being attended with results so different from those of other experimenters. When an animal is introduced into a vessel of inflammable air, its whole body is exposed to that air; and it is not yet known by philosophers what disorders that fluid may occasion to the animal frame. It is true that none are observed to be produced by other noxious kind of air; but if it be considered, that the vapours of sulphur make a great impression upon frogs, even when those animals do not breathe them, but have their *aspera arteria* tied up, it will not seem impossible for the inflammable air, in some manner or other, to act upon the body of animals. It may, perhaps, hinder the perspiration; it may insinuate itself through the pores of the skin; in short, its action upon the body seems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the inflammable air through the mouth only, without immersing their whole bodies into it. I chiefly used bladders tied to their mouths, but sometimes I also made use of tubes which entered immediately into the wind-pipe. In both cases the animals died in a very short time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the *body* of an animal; for I kept some of them immersed in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being in this manner ascertained, that the inflammable air could not be breathed by animals with impunity, it still remained to find out the cause of Mr. SHEELE's mistake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. SHEELE. The inflammable air used in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inside, but a little moist on the outside. The quantity of air contained in each bladder was about eighty cubic inches. The air coming out of the matras passed through about one inch of water before it went into the bladders. At first I
breathed

breathed the inflammable air with a kind of fear; but finding that it occasioned no painful impreſſion, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was ſtill inflammable, and being tried with the teſt of nitrous air it gave II-28, III+20.

Before I go farther I muſt explain the formula which I uſe to expreſs the diminution of reſpirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glaſs tube of about eighteen inches in length, and half an inch in diameter, cloſed at one end, and of a conſtant diameter throughout its whole length: this tube has a mark at every three inches, which marks or diviſions I call *meaſures*, and every inch is divided into twenty equal parts; ſo that every *meaſure* is divided into fixty portions, which I call *parts*. Into this tube, by means of an inſtrument which meaſures always one conſtant quantity of air equal to one meaſure of the tube, I introduce two meaſures of reſpirable air and one meaſure of nitrous air, after which I meaſure the diminution; then I introduce a ſecond meaſure of nitrous air, and again meaſure the diminution. The whole meaſures I expreſs in Roman characters, and the

Z z 2

parts

parts of a measure I express in common numbers; for instance, when I say $11-16$ and $11+10$, the first expression means, that after having introduced into the tube two measures of common air, and one measure of nitrous air, the space occupied by the mixture of these two airs was two measures - 16 parts, or 60ths of a measure; and the second expression shews that, after having introduced another measure of nitrous air, the space occupied was two measures + 10 parts. The reason and particulars of this method will be given hereafter in a paper expressly written upon the method of determining the degree of the salubrity of the air by means of nitrous and inflammable air.

Having introduced eight cubic inches of common air into the same bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it thirty-four times successively, and afterwards found it very much altered, so that it extinguished a light many times successively. An animal introduced into a vessel of that air immediately gave signs of uneasiness: and the air being tried with the nitrous air gave $11+20$, $11+15$; whereas, before it had been breathed, it gave with the same nitrous air $11-15$, $11+18$.

This

This experiment shews, that the air which remained in the bladder in the first experiment was not so good as that breathed thirty-four times successively. In order to make this experiment with more precision, I breathed eighty cubic inches of common air, introduced into the same bladder, only eleven times; beginning after a natural expiration. Then I examined this air with the nitrous air, and found that it gave II—13, III+28.

Hence it is plain, that the mixture of inflammable and pulmonary air breathed eleven times is much inferior to common air breathed an equal number of times; so that there can remain no doubt but that inflammable air is at least worse than common air.

Willing, however, to ascertain this matter still better, I tried to breathe it immediately through a large receiver, partly immersed in water, and swimming in it, so that the air within the receiver was of the same elasticity with the external air. For this experiment I made use also of a glass tube bended in two different directions.

The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than three times, and even at the second inspiration I felt a great oppression. As these experiments can be depended upon, because they were often and at different times repeated, there

there seems to be reason enough to suspect, that the bladder might possibly alter the nature of inflammable air, and render it more fit for respiration, notwithstanding that the mere contact of the bladder seemed not sufficient to produce such an effect, which is irreconcilable with other facts: yet some reason must certainly exist sufficient to explain Mr. SHEELE's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. SHEELE's manner eleven times successively, I not only breathed it without any inconvenience, but observed that the first inspirations were even pleasing; more so than when I breathed common air. I felt a facility of dilating the breast, as if the air was as light as that at the top of high mountains. I never felt a like sensation, even when I have breathed the purest dephlogisticated air. I do not think that I was mistaken in these sensations, or gave a loose to imagination, because I was rather prejudiced against the inflammable air, after I had seen various animals immediately die in it, and I was rather fearful when I first began to breathe it: besides, this facility of breathing it, accompanied with a pleasing sensation, I have constantly observed in all my experiments upon this subject.

This

This pleasure, however, I paid very dear for in another experiment, in which I was near losing my life. Having filled a bladder of the largest sort with about 350 cubic inches of inflammable air extracted from iron filings through water, which air was not at all diminished by the mixture of nitrous air; I began to breathe it boldly (owing to the encouragement received from the above related experiment), and resolved to breathe it as long as my strength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmospheric air. Having made the first inspiration I felt a great oppression upon my lungs. Towards the middle of the second inspiration I heard Mr. CAVALLO, who favoured me with his assistance in these experiments, say, that I was become very pale: by this time the objects appeared confused to my eyes. Notwithstanding this, I made the third inspiration; but now my strength failing, I lost my sight intirely, and fell upon my knees. In this situation I breathed the air of the room, but my knees not being able to support me, I fell intirely upon the floor. However, in a short time I came to myself, so as to be able to get up, &c.; but my respiration continued to be affected with difficulty and pain, as if I had a great weight upon the breast; nor did I perfectly recover before the next day.

It must be observed, that during this experiment I kept my nose close stopped.

This same inflammable air contained in the bladder, which I had breathed three times, was examined in various manners, and was found to be as inflammable as before; it exploded as usual, when mixed with dephlogisticated air, but after having been shaken in water for a short time, being tried with the nitrous air, it gave III-10, IV-10, whereas before it was not at all diminished. At this time the common air, with the same nitrous air, gave II-14, II+10. Hence it appears, that the inflammable air, after being breathed, is rather better than before, because in that case it is a little diminished by the addition of nitrous air.

In order to ascertain whether this alteration was occasioned by the bladder or no, I made the following experiment, which, having been often repeated, was constantly attended with the same event. I introduced into a bladder, which was sometimes moist and sometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that situation for some minutes, beating in the meanwhile the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air ob-

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served, that it suffered no diminution, exactly as it suffered none before it had been put into the bladder.

Having ascertained, in this manner, that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of ascertaining Mr. SHEELE's experiments, and of understanding why I had been able to breathe it eleven times, than by supposing that the air of the lungs, which can never be thoroughly emptied by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about thirty-five cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inspiration, the air expelled may amount to sixty cubic inches. These forty inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after that the lungs have been thoroughly emptied. In the experiment above related, of the three inspirations I made into the inflammable air, it may be easily supposed, that twenty inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, although it is itself partly phlogisticated, is however diminished by nitrous air; and when it stands in the bladder it is nearly equal to $\frac{1}{17}$ th of the inflammable

air of the bladder breathed three times; hence this lost ten parts by the mixture of nitrous air.

This explanation, which it is necessary to adopt after having exploded all the other hypotheses, is very analogous to the above related experiment of the smaller bladder filled with inflammable air which was breathed eleven times successively. This air was breathed after a natural expiration, so that there still remained in the lungs about seventy-five inches of common air. These seventy-five inches of pulmonary air, together with the eighty inches of inflammable air, were mixed together during the eleven inspirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and, accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed eleven times) than the air of the large bladder breathed three times only, after the lungs had been emptied as much as possible.

All the other experiments that I have made in confirmation of this hypothesis seem universally to favour it. If a Guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a small bird into only fifty inches of it, and they be left therein till they are dead, that air afterwards will not be sensibly diminished

nished by nitrous air; but if a much larger animal is introduced into the 400 inches of inflammable air, or a small animal into a few cubic inches of that air, then it will be found to be sensibly diminished by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is so much the less as the animal is smaller.

Mr. SHEELE says, he found that the inflammable air after being breathed some time intirely loses its inflammability; from whence he concludes, that the lungs, instead of imparting some phlogiston to, imbibe it from, whatever substance it can be extracted. Though all the direct experiments which shew that a phlogistic principle is continually detached from the lungs, and joins itself to the common air, were wanting, still Mr. SHEELE's consequence could not be drawn, because the experiment is not true. With respect to my own experience I may safely say, that I have always found it inflammable in every circumstance, even after I had breathed it eleven times successively: and I have not only found it inflammable in the bladder, but I have fired it in the act of let-

ting it out of my mouth. In this manner a flame may be produced from the mouth, of various inches in length, and two or more inches in breadth.

But whence does that sensation of levity and facility of breathing the inflammable air, which I have described above, originate? At present I can only have recourse to a mere mechanical cause for a solution, for I do not observe in inflammable air any property that seems capable of altering the lungs upon a chemical principle; neither have I observed any decomposition of air, or alteration of the fluids of the animal. It has been observed, that inflammable air, after being breathed, comes out of the lungs with the same properties it had before. It is also known, that inflammable air is not sensibly absorbed by water, at least after a short time. The lungs, or more properly the pulmonary vesicles, are continually moistened with fluids; but that air cannot be absorbed by them, except it be first decomposed. Nothing else therefore remains to which we can have recourse for an explanation of the above mentioned sensations, but the well known levity of the inflammable air compared with common air. And indeed the sensation I felt when I breathed that air, is like that of a very light fluid which does not oppress the lungs, and is hardly felt. This explanation agrees exactly with
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some experiments I have made with common air rendered more light by fire. This air I have found may be breathed much easier, although not for so long a time, as when it is more condensed. It must be said, indeed, that this is occasioned by another particular cause, which has nothing to do with the case of the inflammable air, and which cannot be properly examined in this place.

After all, it still remains to be known, why inflammable air, which kills animals so soon, may be breathed without any oppression, when in a small quantity, *viz.* when it is mixed with common air; and the following experiments, which are very analogous to those related above, will shew that the question is not uninteresting.

I introduced 350 cubic inches of common air into a bladder, and after having made as strong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained forty times successively. Afterwards, having taken the air out of the bladder, I found that it extinguished a light several times successively. It formed various crystals with the oil of tartar, but after a very considerable time; some of these crystals had the shape of needles, others were like flowers: being tried with the nitrous air, it gave II-18, III+18. This air, therefore, was very much phlogisticated, nor could I possibly have breathed it longer than I did, without

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out falling upon the ground, as I already felt my strength failing, and the objects appeared confused before my eyes. Into ten cubic inches of this air I introduced a small bird, which, as soon as it began to breathe it, made various contortions with its body, and seemed to suffer a great deal. It died in ten minutes time; whereas another little bird introduced into a like quantity, that is, into ten cubic inches of common air, lived in it fifty-two minutes, nor did it shew any sign of uneasiness before it had been in five minutes.

It remains to be accounted for, why the bird could breathe for five minutes longer in the air of the bladder than a man could. It will be sufficient to consider, that when a man in this experiment has made the last expiration into the bladder, he is in a state of pain, and his lungs are loaded with a superfluous quantity of phlogiston, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, besides its being in vigour, has a quantity of common air in its lungs. This seems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I stopped the neck of the bladder with my finger, then breathed the common air several times; and afterwards putting the neck of the
same

same bladder to my mouth again, I breathed that very same air four times successively. Now there is no doubt but that a bird could have breathed it much longer: the reason of which diversity seems to be the following, *viz* that a small bird is in want of a small quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more easily noxious, and unfit for respiration. From all which it may be concluded, that we are in want of a certain quantity of common air necessary for respiration, and for the support of life; and that this air, after being inspired, comes out of the lungs less fit to be breathed a second time.

It has been observed, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as possible; but that it may be breathed when the lungs are in a natural state, in which state a quantity of common air, equal to about forty cubic inches, is known to exist in the lungs of an adult person. This pulmonary air is not infected so far as to be incapable of being breathed various times, and of supporting life. After having made a natural expiration I have with force expelled from my lungs about thirty inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe eight times successively,

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but never longer. It is true, however, that I breathed it with some oppression, even from the beginning, which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural state.

And now it seems no longer difficult to give an answer to the question proposed above, and to account for the small difference observed in the breathing of the two different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed safely, because there is a quantity of common air sufficient for various inspirations, and that the mixture of the two airs may be breathed till this common air is thoroughly infected. But the inflammable air itself is not altered nor decomposed by the respiration. Wherefore we must conclude, that the inflammable air is not such a kind of air as can by itself alone be directly useful for respiration. It must rather be considered as if there was nothing of that air in the case of the bladder; and indeed it is found by experience, that the pulmonary air itself may be breathed eight or nine times in an empty bladder. The not being able to breathe it eleven times successively, as was done when there was inflammable air in the bladder, and the feeling an oppression in the first case and not in the second, must be intirely attributed to the want of thirty-five cubic inches of air expired,

expired, which are necessary to give the lungs all the necessary expansion; whereas, in the other case, the inflammable air serves to fill up space, and, together with the common air, contributes to fill the lungs; so that the inflammable air, considered under these circumstances, and under this point of view, may be said to be useful for animal respiration. This explanation seems most evidently demonstrated by the following experiment. If thirty-five cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural state, it will be found, that one may breathe it for twenty times or longer; whereas, when the bladder was empty, it could not be breathed more than nine times at most.

Before I finish this paper it will be proper to mention another cause, which, perhaps, also contributes to render the inflammable air of the bladder less noxious: this is the levity of the inflammable air itself with respect to common air, which hinders the inflammable mixing with the common air. The inflammable air swims continually upon the common air, just as æther swims upon water; and the inflammable air swims still better than æther, because it is much lighter in comparison than æther. Various experiments made upon volatile substances have convinced me of this truth. If equal quan-

tities of common and inflammable air, or dephlogisticated and inflammable air, are put into a tube, and two birds are introduced in it, so that one of them may stand at the top, and the other at the lower part of the inverted jar; it will be found, that the first mentioned of these birds not only will die considerably sooner than the other, but will shew signs of uneasiness as soon as it is come to that place.

The inflammable air, therefore, when breathed together with a considerable quantity of common air, must always swim at the top of it, filling the cavity of the wind-pipe, &c. while the common air occupies the lower place, and filling the smallest pulmonary vesicles is subservient to the ordinary functions of the lungs.

Here I put an end to my observations upon inflammable air considered with respect to respiration; but I beg leave to add a few words respecting a property of the inflammable air, which, as far as I know, has been overlooked by the most diligent observers.

I mean here to speak of such inflammable air as is extracted from metals, by means of oil of vitriol, especially that extracted from iron and zinc. The air of these metals, when presented to the flame of a candle, not only burns with a whitish flame inclining to green (as is well known); but exhibits a kind of sparks or explosions which

which may be easily distinguished between the body of the flame by their vivid light. These sparks, which are of a vivid colour, dart in every direction. They might be easily taken for those sparks that are emitted from red-hot iron; or they might be compared to very small grains of gunpowder, if these were inflamed successively, and without smoke; or they might even be compared to charcoal that sparkles, but without any noise. This phenomenon seems very interesting, as it respects the nature of the inflammable air itself. What seems to me most singular is, that this appearance forms a distinctive character between the inflammable air of metals, and that extracted from animal or vegetable substances; at least I may safely say, that I never found the inflammable air of animal or vegetable substances sparkle like that extracted from metals. In several of the former kinds of air I could observe no sparkling at all; in others the sparks were so few that they might be considered as nothing in comparison to the sparkling of the inflammable air from metals.

The inflammable air of metals itself, if left in contact with water for a long time, or shook in it till it becomes less inflammable, will in great measure lose its sparkling property; and at last loses it intirely, when it is become in a state of being hardly inflammable. I have observed,

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that the inflammable air is the more difficult to be decomposed, by being shaken in water, as the number of the sparks it shews when burning is greater; and according to this number of sparks, the inflammable air makes greater or weaker explosions when mixed with the dephlogisticated air; so that it seems proved by experiments, that the phlogistic principle is more fixed and in greater quantity combined with the inflammable air of metals, than with that of vegetable or animal substances. I do not mean to deny the possibility of finding other species of inflammable air extracted from other substances besides metals which may explode like that extracted from metals; but I only say, that in those cases the inflammable air will also sparkle more, and will be found less easy to be decomposed by water. There are other substances that give the inflammable air in great quantity, and which cannot be considered as animal or vegetable substances, but come rather near the nature of metals; as, for instance, the spathose iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a matrafs. But the metal in this substance is not in its pure state, and it may be considered rather as a calx of iron than true iron. Accordingly, this air can hardly sparkle at all; it explodes more like

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the inflammable air of vegetable or animal bodies than that of metals, and it is easily decomposed in water.

This property of inflammable air of metals which I have discovered, throws great light upon the analysis of the decomposition of that air which I have made in two different ways. The first is to fire it together with common or dephlogisticated air, in vessels filled with very pure quicksilver, and also in vessels filled with distilled water. The second method is to decompose it by shaking it in pure distilled water. In the first process a great number of experiments are required in order to obtain a sensible residuum; besides, the igneous part is lost. The second method requires an exceedingly long time, but it is the most complete; for which reason I have used it for the decomposition of other kinds of air.

XXV. *On the Variation of the Temperature of boiling Water.* By Sir George Shuckburgh, Baronet, F. R. and A. S. and Member of the Academy of Sciences and Belles Lettres at Lyon.

Read March 11, 1779.

THE heat of boiling water having for some years been used as one of the terms for graduating the scale of thermometers; together with the particular attention the Society has lately given (*vide* the Report of the Committee, Phil. Transf. vol. LXVII.) to this branch of inquiry, and I may add the singular success with which this age and nation has introduced a mathematical precision, hitherto unheard of, into the construction of philosophical instruments, will render it unnecessary for me to say more in respect of the following experiments, than simply to lay them before the Royal Society.

That the heat of boiling water was variable, according to the pressure of the atmosphere, seems to have been known to FAHRENHEIT as early as the year 1724^(a).

(a) *Vide* Phil. Transf. N° 385. wherein is proposed a curious project of determining the weight of the atmosphere by means of a thermometer alone, under the title of "Barometri novi descriptio."

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A few years after this, Messieurs LE MONNIER and CASSINI (*Mem. de l'Acad. des Sc. pour 1740*) made some decisive observations, to shew that this quantity was very considerable. It was left, however, for Mr. DE LUC to make a much more compleat series of experiments, which he has described and reduced into system in his *Recherches sur la Variation de la Chaleur de l'Eau bouillante*. It remained only that these should be verified. Towards the latter end of the year 1775 I had an opportunity of repeating these observations with a small pocket thermometer of about six inches long, made by Mr. NAIRNE; an instrument, it must be confessed, not very accurate for such an examination, but with which I thought, however, I could observe to within a quarter of a degree; my object at that time, amidst a variety of other philosophical pursuits, being to assure myself that the variation took place, rather than critically to examine the quantity of it. I shall relate these observations, as the result of them upon my return to England led me to some more accurate.

Table

Table of observations of the boiling-point, made in a journey over the Alps.

Place of observation.	Height of the barometer.	Heat of boiling water by observat.	Heat of boiling water by Mr. DE LUC's thermometer	Diff.	Difference in $\frac{1}{100}$ of total.
	Inches.	°	°	°	
Bologna,	30.21	213.5	213.5	-0,0	0
Geneva,	28.60	210.4	210.9	-0,5	-16
Modane,	26.61	207.3	207.4	-0,1	-2
Lannebourg,	25.75	205.1	205.9	-0,8	-9
Mount Cenis,	24.03	201.2	202.6	-1,4	-11
Ditto,	23.91	201.1	202.4	-1,3	-10
Mean					$\frac{0}{100} = \frac{1}{11}$

The second column gives the height of the barometer at the time of observation; the fourth, the heat of boiling water deduced from Mr. DE LUC's rules, compared with the lowermost observation, or that under the greatest pressure; the sixth gives the difference between the theory and the experiment in the motion of the boiling point in hundredth parts of the whole space described: from whence it might be concluded, that the motion or variation of the boiling point with a given variation in the pressure of the atmosphere was $\frac{0}{100}$ or $\frac{1}{11}$ greater

greater than by the theory alluded to^(b). But these were but gross experiments, and perhaps unworthy of such a competition. They induced me, however, to make the following. In the beginning of last year (1778) with the assistance of Mr. RAMSDEN, I procured a most excellent thermometer, every way adapted for this purpose. It was about fourteen inches long, but the interval between freezing and boiling only $8\frac{1}{4}$ inches^(c), and though every degree was something less than the $\frac{1}{10}$ th of an inch, yet, by means of a semi-transparent piece of ivory, which applied itself close behind the glass tube, sliding up and down in a groove cut in the brass scale for that purpose, carrying a hair-line division, at the extremity of which was a vernier dividing each de-

(b) The same instrument immersed in snow just melting at the top of Mount Denis fell to 32° , the point of freezing observed at the level of the sea.

(c) It may possibly be suggested, that if this interval had been greater, *viz.* 20, 30, or 40 inches, I should have had a much larger scale and more convenient instrument; but in this, as in most other mechanical contrivances, our progress beyond certain limits is prevented; for if the perpendicular height of the column of quicksilver be much increased, the weight of it will be such as to distend the ball, and the instrument may differ from itself in a vertical and horizontal position by half a degree, as I have seen in a tube only fifteen inches long; and if this circumstance be endeavoured to be corrected by making the bulb of the thermometer thicker, its sensibility will be proportionably diminished. If my experience were to lead me to conclude any thing, I should consider a tube of a foot long as a *maximum*, and the bore of such a diameter as to admit a ball of a quarter or one fifth of an inch.

gree into ten; with, moreover, a lens of an inch focus; this apparatus being made moveable first by the hand, and more delicately by means of a micrometer screw, whose head was divided into twenty-five divisions, each equal to the fortieth of a degree (for so truly cylindrical was the tube, which had been with care expressly selected from a great quantity of glass, that the divisions in the neighbourhood of the freezing point did not differ from those near the boiling point by so much as $\frac{1}{40}$ th of a degree, and this variation appeared in other parts of the tube strictly uniform, as was found by breaking the column of mercury); by means I say of this apparatus I was enabled to read off any height of the thermometer to within $\frac{1}{50}$ th of a degree. The vessel, in which the water was boiled, which was always spring water, was a cylindrical tin pot, 13 inches high and $4\frac{1}{4}$ inches wide, with a top something resembling that described in Mr. DE LUC's work, contrived to carry off the steam without incommoding the observer, with a waste pipe for the superfluous water in boiling, which might otherwise fall upon the fire and extinguish it. The ball of the thermometer was immersed to within $2\frac{1}{2}$ inches of the bottom of the vessel, and $10\frac{1}{2}$ inches below the surface of the water, so that as near as might be the whole column of mercury was exposed to the heat of boiling water, there

there being only 15° or 20° of the scale, equal about $\frac{1}{50.0}$ part rising out of the water exposed to the temperature of the steam, which in one or two experiments was found to be 180° or 190° , so that the correction for this defect of heat would only amount to a very few hundredths of a degree, perhaps about ,04 or 05, which, as the instrument was exposed to the same circumstances as near as might be in all the observations, I have taken no notice of. I thought it necessary to say thus much respecting the precision of the instrument and the apparatus, and shall now relate the observations at length.

Table of the observations.

° Z	Place of observation.	Height of the baro- meter.	Therm. at Barome- ter reduc- ed to the heat of 50° .	Boiling point.	Mean boiling point.
		Inch.	° Inch.	° 40ths.	° 100ths
1	Summit of Snowdon in Car- narvonshire, Aug. 15, 1778,	26,487	46	26,498	207,07
				207, 8 206,38	
2	Top of Carn Cwm Gafr, Aug. 16, 1778,	27,274	62	27,241	208,64
				208,27	
				208,24	
				208,28	
				208,19	
				208,29	
3	In the descent from ditto,	27,957	51	27,954	209,87
				209,28½	
				209,37	
				209,39	
				209,32	
				209,32	
				209,40	

Table of observations continued.

Number.	Place of Observations.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths.
4	Shuckburgh, Dec. 3, 1778,	28,360	44	28,377	210,17½ 210,20½ 210,20½ 210,21½	210,50
5	Ditto, Oct. 23, 1778,	28,690	47	28,699	211, 8 211,10 211,11 211,13 211,12 211,10	211,27
6	Ditto, Sept. 27, 1778,	28,910	54	28,898	211,19½ 211,20½ 211,24½ 211,17½ 211,14½	211,50
7	Ditto, Nov. 7, 1778,	18,990	47	28,999	211,23 211,24 211,25 211,23	211,60
8	Llanberis, Carnarvonshire, } Aug. 13, 1778,	29,477	60	29,447	212,22	212,55
9	London, Jan. 27, 1779,	29,790	45	29,805	212,35 212,40 212,42 212,36 212,36 212,38 212,39	212,95

Table

Table of Observations continued.

Number.	Place of observation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
10	London, Jan. 28, 1779,	29,990	44	30,008	213, 5 213, 8 213, 10 213, 9 213, 18 213, 10 213, 6 213, 6 213, 11 213, 12 213, 8	213, 22
11	Ditto, June 10, 1778,	30,250	64	30,207	213, 20 213, 23 213, 24 213, 26 213, 23½	213, 58
12	Ditto, Dec. 27, 1778,	30,468	43	30,489	214, 5 214, 9 214, 6 214, 2 214, 5 214, 7 214, 7 214, 5	214, 15
13	Ditto, Dec. 24, 1778,	30,750	46	30,763	214, 18 214, 12 214, 15	214, 37

Table

Table of observations continued.

Number.	Place of observation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
14	London, Dec. 25, 1778,	30,838	47	30,847	214,35 214,32 214,33 214,34 214,35 214,31 214,33	214,83
15	Ditto, at the Adelphi Wharf, 3½ feet above high water, Dec. 26, 1778,	30,948(d)	47	30,957	214,35 214,39 214,40 214,38½ 214,41 214,37 214,41 214,40 214,40 214,34	214,15

The numbers in the fifth column are the corrected heights of the barometer reduced to one and the same temperature, *viz.* 50°, which was necessary in order to have the true proportion of the pressure of the atmosphere, whose influence seems to have so considerable a

(d) This is the greatest height of the barometer that I have ever known, and, as far as I have been able to collect the highest point, that it has ever been seen to stand at in any country where observations have been made and recorded, since the first invention of this ingenious instrument.

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there in the heat of boiling water. Column the sixth shews the height the thermometer stood at in water boiling very fast in degrees and 40ths of a degree; where it will be seen, that the observation was repeated, as the heat is given of every separate trial, which is, perhaps, the best criterion of the confidence that is to be placed in the mean result, shewn in column the seventh expressed in hundredths of a degree.

Having collected this series of experiments, I was anxious to see how far they corresponded with Mr. DE LUC's, and upon comparison of N° 1. and N° 15. I found that the decrease of the boiling heat was $\frac{47}{100}$ greater than the rules admitted of from an alteration of the pressure of the atmosphere of $4\frac{1}{2}$ inches. This difference led me into an examination of all my observations, to see how far they were consistent with themselves; how far they disagreed from Mr. DE LUC's; and, lastly, what general conclusion might be drawn from them.

To avoid in some measure, or at least correct, the errors of observation, the mean of N° 1. and N° 2. the mean of N° 6. and N° 7. and of N° 14. and N° 15. was taken instead of either observation separately; the first and third of these means as two extreme terms, and the second as an intermediate one: with these it was very easy by interpolation or proportion to deduce any other

intermediate term, and consequently from the mean of six to examine all the fifteen observations. Thus, on a comparison of N° 1. and 2. with N° 14. and 15. the mean motion of the boiling point in that interval for one inch of the barometer (*viz.* when the mercury stands at 28,886 inches) is = $1^{\circ},743$; according to Mr. DE LUC this is $1^{\circ},65$. By a similar comparison of N° 1. and 2. with N° 6. and 7. the mean motion of the boiling point in that interval (*viz.* at 27,908 inches) for one inch is = $1^{\circ},779$; by Mr. DE LUC = $1^{\circ},73$. And, lastly, comparing the mean of N° 6. and 7. with the mean of N° 14. and 15. the mean motion of the boiling point in this interval (*viz.* at 29,925 inches) for one inch is = $1^{\circ},709$; by Mr. DE LUC = $1^{\circ},59$. It should follow then, that, within the limits of my experiments, the alteration or motion of the boiling point is greater ^(e) by $\frac{1}{18}$ than from that gentleman's observations, that the heat of boiling water is not directly in the simple ratio of the height of the barometer, nor yet is the progression so rapid as Mr. DE LUC observed it. It may be somewhat satisfactory to see the observations collated and compared.

(e) It is true, that my observations in Savoy give this difference $+\frac{1}{18}$ those of Mr. LE MONNIER equal $+\frac{1}{12}$ (Vide *Recherches sur l'At.* § 964.); and though, perhaps, neither the one nor the other are entirely unexceptionable, they tend, however, something to confirm, although alone they may be unable wholly to support, such a supposition.

N ^o	Height of the barometer.	Boiling point by observation.	Boiling pt. by Mr. DE LUC's rules.	Error.	Boiling point by interpolation from N ^o 1, 2, 6, 7, 14, and 15.	Error.
	Inch.	°	°	°	°	°
1	26,498	207,07	208,54	+0,47	207,18	+0,11
2	27,241	208,64	208,84	+0,20	208,82	-0,12
3	27,954	209,87	210,03	+0,16	209,80	-0, 7
4	28,377	210,50	210,81	+0,31	210,56	+0, 6
5	28,699	211,27	211,34	+0, 7	211,11	-0,16
6	28,898	211,50	211,67	+0,17	211,47	-0, 3
7	28,999	211,60	211,85	+0,25	211,64	+0, 4
8	29,447	212,55	212,74	+0,19	212,42	-0,13
9	29,805	212,95	213,15	+0,20	213,03	+0, 8
10	30,008	213,22	213,47	+0,25	213,49	+0,27
11	30,207	213,58	213,79	+0,21	213,71	+0,13
12	30,489	214,15	214,23	+0, 8	214,19	+0, 4
13	30,763	214,37	214,66	+0,29	214,65	+0,28
14	30,847	214,83	214,79	-0, 4	214,79	-0, 4
15	30,957	214,96	214,96	,0	214,96	,0

I shall make no deductions from this comparison, but leave them to the leisure of the reader. It will, however, probably be inquired, how the thermometer came to stand at 213° in boiling water, when the barometer was about 30 inches, 212° being the degree by which that heat is expressed on FAHRENHEIT's scale? The answer is easy: it was an error in the making of the instrument, and, I believe, a pretty general one. There was

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also a similar error of something more than one quarter of a degree in laying down the freezing point so that the fundamental interval between freezing and boiling, when the barometer stands at 29,8 inches (the mean height at London) was $180^{\circ},71$ instead of 180° ; by this means each division was in fact $\frac{1}{280}$ th part less than a degree: this small correction may therefore easily be applied, if thought necessary, but I have taken no notice of it.

I will now add a general table for the use of artists in making this instrument, both according to my own observations, and those of Mr. DE LUC, that the preference may be given as it shall be thought due; not that it is a matter of any great consequence which is made use of under small variations of the atmosphere; but even under these circumstances, I flatter myself, that the object of this paper will be sufficiently obvious to all who wish to verify a new theory, or aim at accuracy in these days of precision.

Height

Height of the barom.	Correct. of the boiling point.	Diff.	Correct. according to Mr. DE LUC.	Diff.
26,0	-7,09		-6,83	
26,5	-6,18	,91	-5,93	,90
27,0	-5,27	,91	-5,04	,89
27,5	-4,37	,90	-4,16	,88
28,0	-3,48	,89	-3,31	,87
28,5	-2,59	,89	-2,45	,86
29,0	-1,72	,87	-1,62	,83
29,5	-0,85	,87	-0,80	,82
30,0	0,00	,85	-0,00	,80
30,5	+0,85	,85	+0,79	,79
31,0	+1,69	,84	+1,57	,78

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XXVI. *Account of a new Kind of inflammable Air or Gass, which can be made in a Moment without Apparatus, and is as fit for Explosion as other inflammable Gasses in use for that Purpose; together with a new Theory of Gunpowder. By John Ingen-Housz, Body Physician to their Imperial Majesties, and F. R. S.*

Read March 25, 1779.

THE important discoveries on different kinds of air have opened a new field for one of the most pleasing and interesting scenes which ever were exposed to the contemplation of philosophers, and therefore could not fail of exciting in almost every lover of natural knowledge a decided curiosity to see the pursuit of such striking novelties, and in many an almost irresistible temptation to imitate them, and to pursue farther the road already so far opened by the Rev. Dr. PRIESTLEY, Abbé FONTANA, Mr. LAVOISIER, and many other learned and ingenious men.

Who can, indeed, without the greatest satisfaction, look (amidst many other objects of admiration) upon the discovery

discovery of that new aerial fluid, which in purity and fitness for respiration so far surpasses the best atmospherical air, that an animal protracts his life five times as long, or even more, in it than in common air of the best quality?

Dr. PRIESTLEY, the first who discovered this wonderful fluid, extracted it from bodies which we should rather have suspected to have contained deleterious exhalations. He afterwards found it existed in many other bodies in which the most accurate observer never found any thing of an approaching nature.

When I consider the immense quantity of this pure aerial fluid, called by Dr. PRIESTLEY with so much propriety *dephlogisticated air*, which exists as it were in a solid state in nitre, in calcined metals, and even in some other of the most common substances, I cannot express the greatness of my expectation, as a physician, from such an important discovery ^(a). I flatter myself, that ere long an easy and cheap method will be discovered by which such quantities of this beneficial air may be ob-

(a) Since Dr. PRIESTLEY's last publication on air, he discovered, that the same water which, if examined immediately, gives only a small quantity of bad air, yields spontaneously about ten times the quantity of pure dephlogisticated air after standing some time exposed to the sun. Of this I was an eye-witness. The important discoveries of Abbé FONTANA upon this subject, which he shewed me when I was last in Paris a year and a half ago, will soon be published by himself. He extracted this wonderful aerial fluid from different kinds of water by boiling them over a fire.

tained as will serve to cure several diseases which resist the power of all other remedies, and so prolong, as it were, human life. We may expect, with some degree of confidence, that this new element, when it shall be used for the benefit of respiration, will be found more fit than the best common air to free our body from that quantity of phlogiston or inflammable principle which seems to exist sometimes in too great a quantity in the mass of our blood; or from which it seems sometimes, as it were, to be let loose in too great abundance, producing, perhaps, in consequence fevers and other symptoms, the causes of which have not yet been clearly elucidated by the best medical writers.

This dephlogisticated air, free from the inflammable particles with which the best common air is always infected, will probably be found capable of absorbing a greater quantity of those phlogistic particles with which the air coming from our lungs is always found to be pregnant, and thus of ventilating, as it were, much more expeditiously the mass of our blood of that which a constant exertion of the organs of respiration is not always able to free it from in a sufficient quantity.

These important pursuits have led Dr. PRIESTLEY to the discovery of one of the benefits, and perhaps the principal, we derive from respiration; that function of the
animal

animal oeconomy which is so important, that without its constant influence an animal, once born, has but a few moments to live.

The criterion of the degree of goodness of respirable air, by the quantity which is absorbed or destroyed by the addition of nitrous air, is one of those useful discoveries of Dr. PRIESTLEY'S from which mankind will, perhaps, hereafter reap a considerable benefit.

The discovery of the various kinds of inflammable airs or gasses becoming powerfully explosive, when they are mixed with a sufficient quantity of common air, and still more so when they are combined with dephlogisticated air, is one of those improvements in natural philosophy which, giving occasion to various amusing and interesting experiments, have cast at the same time a new light upon some powerful agents, whose mischievous force was known, though their nature was still in the dark.

As those inflammable airs have been of late years one of the principal philosophical amusements, I intend to lay before the Royal Society an easy method of producing, without any trouble or particular apparatus, such quantity of an inflammable air or gas as may be required; to which I shall subjoin some considerations which have occurred to me about the nature of that wonderful substance gunpowder, which conveys death at such an immense distance by its almost irresistible explosive force,

force, by which it propels metallic balls of a considerable weight. To investigate the nature of this awful ingredient, by which the fate of empires is decided in our days, cannot be indifferent to any body who takes delight in investigating natural causes.

Being at Amsterdam in November 1777, Messieurs AENEAE and CUTHBERTSON, two ingenious philosophers of that city, were so good as to shew me some curious experiments with explosive and inflammable airs of different kinds. They produced an inflammable air, by mixing together equal quantities of oil of vitriol and spirit of wine, and applying heat to the phial containing the compound. A great quantity of white vapour was extricated, which, passing up the inverted receiver filled with water, settled at the top and depressed the water, as other airs do. This air soon became clear, the white fumes being absorbed by the water. This air was easily lighted in an open cylindrical glass, and burnt almost as clear as a candle, the flame descending gradually lower and lower till it reached the bottom. A very little quantity of this air mixed with common or dephlogisticated air, for instance, one fourteenth or one tenth part, and kindled by an electrical spark, exploded with a very loud report, and shattered the glass to pieces in which it was kindled, when it did not find a ready vent.

They

They had contrived a kind of a pistol for the purpose, consisting of a strong cylindrical glass tube with a piston adapted to it. To the end of this tube was fixed a brass barrel, like that of a common pistol: into this barrel a brass bullet was put loose, so that the barrel was placed a little above the level, to prevent the bullet rolling out. The barrel was directed to a board of oak at eight or ten feet distance. A proper quantity of common and inflammable air (produced in the manner above mentioned) being drawn into the glass tube by means of the piston, it was fired by directing an electrical explosion through it. The explosion was very loud: the ball hit the board with such a force that it made a strong impression in it, and recoiled with a considerable force, so as to hit the wall behind us, and to put us in some danger of being hurt by its rebounding force.

The same gentlemen told me, that this inflammable air had in some respects the advantage over the inflammable airs extracted from metals by the vitriolic or marine acid, and that extracted from mud or marshes; because this air being heavier than either of these airs, and even than common air, is not so easily lost out of an open vessel; and, that when it escapes into the open air, it agreeably perfumes the room with the smell of *spiritus vitrioli dulcis* or æther; whereas the other inflammable airs,

which from their less specific gravity escape easily into the common air, yield an offensive, disagreeable stench.

Mr. AENEAE, having examined the specific gravities of the different inflammable airs compared with common air, favoured me with the following result of his inquiries.

A vessel, which contained the weight of 138 grains of common air, contained 25 grains of inflammable air extracted from iron by vitriolic acid, and 92 grains of inflammable air extracted from mud or marshes, and 150 grains of that extracted from oil of vitriol and spirit of wine.

I was much pleased with the above mentioned experiment, and immediately thought that the operation of extracting this inflammable air or vapour could be dispensed with by employing vitriolic æther, which in reality is contained in the vapour expelled by heat from oil of vitriol and spirit of wine, which vapour, condensed in the process of distillation, yields æther. But I resolved to defer making the experiment till I should arrive in London, where I intended to make some stay to see my old friends, and acquire what medical and philosophical knowledge I could.

Having arrived in this capital in the beginning of January 1778, I lost no time in pursuing my idea. For

this purpose I poured some drops of æther into a strong glass tube, and directed an electrical explosion from a Leyden phial through it; but, to my mortification, no explosion happened. I then threw a bit of cotton, dipped in æther, into the same tube, but it would not take fire. Though these first trials proved unsuccessful, I was too much persuaded, that in some way or other it must succeed, to be discouraged. I tried it in several different ways, and at last, before the end of January, I succeeded once or twice in producing a loud explosion, by throwing into the tube a little bit of paper dipped in æther. But as the experiment often failed (of which I could then find no reason), I did not venture to shew it to my friends till I had hit upon a method, the certainty of which would prevent my being exposed to some confusion by exhibiting an experiment which was so apt to fail. However, I told Sir JOHN FRINGLE, Mess. NAIRNE and BLUNT, and some few others of my friends, early in the spring, that I had found out a method of firing an inflammable air pistol without being at the trouble of making inflammable air in the ordinary way; as I produced it at pleasure in an instant, without any trouble or apparatus; and that I would shew them the experiment as soon as I was sure of succeeding constantly. In the mean time I continued to produce this air before my acquaintance in

the way I had seen it produced at Amsterdam. Soon after, hitting upon better and surer methods of succeeding, I began to show it to those who came to visit me, and in the beginning of the summer I made no scruple of shewing it to every body.

The reasons why I did not succeed in the beginning I found afterwards to be, either that I employed too great a quantity of æther, or that the air or vapour of the æther was not thoroughly incorporated with the other air; for the same numbr of drops of æther poured into the air pistol, which would not produce an explosion when the pistol was not shaken, made a very loud one when it was forcibly agitated.

The surest method of succeeding I find to be the following: I dip a small glass tube, open on both sides, and the bore of which is one twelfth of an inch in diameter into a phial containing æther, and when two or three drops of the liquid have entered the tube I apply my finger to the upper end of it, to keep the liquor suspended. I take the tube out of the phial, and thrust it immediately into a small *caoutchouc*, or elastic gum bottle: this being done, I withdraw my finger from the tube, and take it out of the *caoutchouc*; thus the little quantity of æther, suspended in the end of the tube, is dropped into the *caoutchouc*, the neck of which is to be immediately

immediately inverted into the orifice of the air pistol, and, after giving it a gentle squeeze, withdrawn out of it: after which, a bullet or a cork is to be thrust into the mouth of the pistol, when it is ready for firing. This whole operation may be performed in the space of five or six seconds.

The considerable force of explosion, and the loud report of the ordinary inflammable airs, induced Mr. VOLTA, of Como, to believe, that these airs might, perhaps, become a substitute to gunpowder. If this expectation had been well founded, the greatest *desideratum* would, I think, have been to find out a way to produce such air at any time without trouble, and to carry it about in as little compass as possible: which two conditions I should have pretty nearly fulfilled, as all the inflammable air requisite for the explosion of the pistols contrived by Mr. VOLTA is contained in the bulk of one single drop of æther; which drop, poured in the pistol itself, is full sufficient to produce a very powerful explosion. But how far this expectation of Mr. VOLTA, as to the use of inflammable air in offensive arms, is well grounded, I will attempt to explain in the subsequent part of this paper.

This inflammable air (which, perhaps, might more properly be called vapour, as it is absorbable by water)

has

has the principal properties of the other inflammable airs, *viz.* it will not inflame but where it is in contact with common or dephlogistified air; and therefore only takes fire at the top of the vessel containing it, the flame going gradually downwards till the whole is consumed, if the vessel is of a cylindrical form, and pretty wide. If it is diluted with common air, but not sufficiently, it will burn as other inflammable air without exploding. Being sufficiently diluted with common air, especially with dephlogistified air, it explodes with a very great report and a considerable force. It is unfit for respiration in a concentrated state, and is as mortal for an animal plunged into it as any other of the inflammable airs; though in a diluted state it seems to be rather pleasant to the organs of respiration.

It differs in some respects from the common inflammable airs; as, for instance, it is much heavier, as is already observed, than any of the other inflammable airs, and even than common air. It does not inflame or explode with so small a spark of electrical fire as the other inflammable airs, requiring the explosion of a Leyden phial to be fired with certainty, though one single inch of coated glass will be sufficient; so that a Leyden phial, containing twelve or fourteen square inches of coating, may fire an air pistol loaded with this kind of air several times,

times, if the charge be divided by taking it out with a small glass tube, fitted up in the manner I described in the paper I had the honour of laying before the Royal Society last year, and which is inserted in the second part of the 68th vol. of the Phil. Trans.; in which paper I describe a kind of an electrical match to light a candle with. This air being in contact with water is absorbed by it in a few days, though the water be not stirred: and much sooner loses its inflammability by contact with water than the other inflammable airs do. However, I found that in such a situation this air had not yet lost all its explosive force in six days, though the water in which the cylindrical glass, ten inches long and one in diameter, was inverted, after I had poured into it five or six drops of æther, had gradually ascended till, on the third day, it reached to the half of the height of the glass: so that it seems as if these drops of æther, by their expanding in the form of air, had forced out half of the common air contained in the glass, in the place of which the water afterwards ascended in proportion as the air or vapour generated by the æther was absorbed by the water.

That this air is specifically heavier than common air, and does not readily incorporate with it, is easily demonstrated by the following experiment. I poured five or six drops of æther into a cylindrical glass, ten inches long and

and one inch in diameter: the æther being soon evaporated, I clapped my hand upon the mouth of the glass, and inverted it to incorporate the air generated by the æther with the common air; after which I left the glass open during half an hour, when I dipped in it a piece of wax taper burning, stuck upon a bended wire. As soon as the taper reached the brim of the glass, a flame burst out with a weak explosion. In a quarter of an hour I again thrust the wax taper into the same cylinder, and no flame was observed till the wax taper reached the place where the flame ended the first time. This second explosion did not set fire to the whole at the bottom of the glass. I again waited a quarter of an hour, and then again thrust the wax taper into it, by which the remainder of this inflammable gas, which had remained settled all that time at the bottom, was exploded ^(b).

(b) It is remarkable, that æther, the most volatile liquor yet known, and so apt to spread itself through the air by a quick evaporation that a drop of very fine æther, which falls from the height of a few feet, is quite evaporated before it reaches the ground, and no glass stopper of itself is able to keep it from evaporating: it is remarkable, I say, that notwithstanding this extreme volatility, the air or vapour, into which it changes by evaporation, should be so far from participating of the same volatility, that it may be kept hours together in an open cylindrical vessel without evaporating, mixing with the common air, or losing its inflammability; so that this substance seems to undergo a sudden metamorphosis, and to change in an instant from the lightest of all liquors to one of the heaviest of aerial fluids, fixed air, which being one half heavier than common air, according to the experiments of Mr. CAVENDISH, is probably much heavier than this æther air.

I found

I found that æther, in which as much urinous phosphorus is dissolved as will make it luminous in the dark, when some drops are poured upon water; is very brisk in taking fire, when employed for an inflammable air pistol; but that the experiment, when repeated, will be apt to fail, because the phosphoric acid which remains in the pistol, and by its nature attracts the humidity of the atmosphere, will soon fill the inside of the pistol with a coat of moisture, and prevent the electrical spark from kindling the inflammable air.

It appeared, that a little camphire dissolved in æther increases its explosive force, and makes it less apt to fail.

As this inflammable air is heavier than common air, it is clear, that the mouth of the air pistol should be kept upwards at the time of charging it; whereas it is better to invert the pistol when the ordinary inflammable airs are employed, which, being specifically lighter than common air, rise of themselves in the pistol when its mouth is placed inverted upon the orifice of the vessel which contains them.

It is true, that the squeezing the elastic gum bottle, when placed upon the pistol, forces some of the inflammable gas out of it, which is lost in the common air; but notwithstanding this waste, the inflammable air which

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remains

remains in the pistol is sufficient to produce a loud report, which is all that is required. Indeed, one single drop of the æther could be easily shaken out of the glass tube immediately into the pistol, without making use of the elastic gum bottle; but this drop, evaporating into elastic air, leaves behind it a good deal of moisture, whether inherent in the æther itself, or attracted from the atmosphere. This moisture, in the way I use to load the pistol, remains in the elastic gum bottle, which is therefore always found moist when the experiment is repeated several times.

It was, indeed, known before this time, that æther and other volatile inflammable liquors spread by evaporating inflammable effluvia through the surrounding air, especially when they are heated; and that these effluvia have sometimes by the imprudent approach of a candle taken fire, and conveyed the inflammation to the liquor itself: but I never heard that any body employed these liquors instead of ordinary inflammable air in communicating to common air an explosive quality, or in firing inflammable air pistols, before I communicated the experiment to my friends

As it will, I think, appear very probable, by what will be said hereafter, that little more than a pleasing amusement can be expected from the force of any inflammable
air;

air; the ready production of such inflammable air always ready for use, when an explosion is intended to be produced, may be of some importance to philosophers whose time must be sparingly taken up.

Comparative view of the expanding force of explosive air and gunpowder.

The force of gunpowder has been ascribed by Sir ISAAC NEWTON and all other philosophers to the sudden extrication of an amazing quantity of elastic permanent aerial fluid within a narrow space incapable of containing it; which quantity ought to be attentively attended to, in order to estimate the comparative power of the two substances.

BENJAMIN ROBINS, whose work, intituled, *New Principles of Gunnery*, passes in this country for a standard book, affirms, that gunpowder, when fired, generates a permanent elastic fluid of 250 times the bulk of the powder before it was fired. He found, that common air, which is heated by the contact of a red-hot iron, expands to four times its former bulk. Hence he concludes, that the elastic air, disengaged from gunpowder, must expand also to four times its dimension; and that it occupies about a thousand times the bulk of the powder in the moment of inflammation.

The learned Count SALUCE remarks ^(c), that the elastic air generated by the inflammation of gunpowder occupies, when cold, 222 times the bulk of the powder, which agrees, as he finds, with the computation made by Mess. HAUKEBEE, AMONTONS, and BELIDOR. This calculation he confirms in the *Melanges de Philosophie et des Mathematiques de la Societ  Royale de Turin*, which is a continuation of the former work. He is of opinion, that this elastic fluid is of the same nature with common air (which was likewise the opinion of Dr. HALES); and that the prodigious force of gunpowder depends upon the action of the fire on all its parts, by which this fluid exercises all the force of its elasticity.

The extrication of such a considerable quantity of a permanent elastic fluid by the firing of gunpowder has been put to a particular use by several philosophers.

The celebrated Mr. DE LA CONDAMINE gives an account of a brass air gun contrived by one Mr. MATY, of Turin; which he loaded with air condensed by firing in it two ounces of gunpowder, which, by its inflammation, let loose such a quantity of air as was sufficient to shoot a leaden bullet sixty paces, and to repeat the process

(B) *Miscellanea Philosophico-Mathematica Societatis privatæ Taurinensis*,
p. 125.

eighteen times, the strength of the explosion diminishing gradually as in other air guns ^(d).

In the learned work of Mr. ANTONI, an Italian gentleman, I find the same experiment, the account of which is accompanied with a figure of such an air gun, in which the author fired one ounce of gunpowder, the barrel being very stout, and of a size capable of containing ten ounces. He afterwards let a quantity of this compressed air out by a valve in the same way as it is done in the common air guns. This one ounce of gunpowder yielded air enough to propel a leaden bullet through a board three lines thick, at the distance of forty paces, and to repeat the process sixteen or eighteen times ^(e).

The difference of the quantity of elastic fluid obtained from the firing of gunpowder by Mr. ROBINS and others might be owing to the difficulties attending the investigation, or to the different proportion of the ingredients used in the composition of the powder; as it is well known, that gunpowder for the use of the army is made of five or six parts of nitre to one of charcoal,

(d) *Extrait d'un Journal de Voyage d'Italie, par M. DE LA CONDAMINE, inséré dans les Memoires de l'Academie Royale des Sciences de Paris, 1757, p. 405.*

(e) *Examen de la Poudre traduit de l'Italien de M. ANTONI, par M. le Vis-
comte de FLAVIGNY, 1773.*

and

and one of sulphur; when seven parts of nitre are used, it is called *poudre d'artifice*.

The celebrated Mr. JOHN BERNOUILLI calculates the density of the air contained in a solid state in gunpowder to be $\frac{1}{1000}$ of what this fluid is when it constitutes a part of our atmosphere. But he does not consider this air as existing in all the component ingredients of the gunpowder, but chiefly in the nitre: and Count SALUCE supposes, that that part of the gunpowder which contains this air constitutes a considerable part of its bulk (though somewhat less than the half). Let us now suppose, that part of the gunpowder which contains this air to be not much less than the half of the whole mass (for it would be difficult to demonstrate accurately to what proportion of the whole mass this part amounts in reality). On this supposition we shall find, that the whole mass of gunpowder contains a quantity of air in a solid state which is reduced in bulk to near $\frac{1}{500}$, or, in other words, that one square inch of gunpowder contains near 500 square inches of air; which being heated in the moment of inflammation will expand to four times its diameter; so that according to this calculation gunpowder must expand in the moment of explosion to near 2000 times its own bulk.

It seems very probable, that this calculation of Mr. BERNOUILLI is much nearer the truth than that of

Mr. ROBINS and Count SALUCE; but yet the evaluation of the two last mentioned writers, though short of one half, proves in reality Mr. BERNOUILLI's calculations to be as just as can be expected, when it is considered, that this evaluation was made before the new discoveries upon the nature of nitre and charcoal. But this assertion will be better understood when I have explained the nature of gunpowder somewhat fuller.

If we continue to say, as we have hitherto done, that the charcoal taking fire decomposes the nitre, and extricates from it that amazing quantity of elastic fluid which was shut up within its substance, we only say what we see in reality is the consequence of setting fire to this ingredient. But this explanation does not convey a clear idea of the manner in which the extrication is carried on; nor of the reason why one single spark of fire, thrown into an immense heap of gunpowder, should almost in an instant spread the conflagration through the whole mass. Neither does it explain clearly, why nitre and charcoal (which separately yield no flame at all, though ever so much heated) when combined and intimately mixed together, explode with as loud a report as a large ordnance piece, surpassing even in loudness thunder-claps; nor why this forcible explosion is accompanied by a most brilliant flame.

Nitre

Nitre is composed of two different ingredients, *viz.* an acid, called from its peculiar nature the *nitrous acid*, and the vegetable alkali. Neither of these two ingredients are capable of inflammation; nay, they even extinguish actual fire. When they are both combined and constitute the neutral salt we speak of, they have not, even by their coalition in one body, acquired an inflammable quality, for nitre may be made red-hot in a crucible without shewing the least appearance of inflammation, not even when a red-hot stone or piece of iron is thrown into it. But if any common combustible substance, as wood, charcoal, or such like, is thrown into the melted nitre, a flame issues with a kind of explosion, though only at the very place where the two substances come into contact. The same flame and explosion is observed when cold nitre is thrown upon a combustible body, in a state of real ignition, on a piece of red-hot charcoal for instance.

The true reason of this wonderful phenomenon has not been considered hitherto with that degree of attention it deserves, and could not have occurred to any body before our modern philosophers had discovered the nature of various kinds of air, and the manner of extracting them from bodies.

Inflam-

Inflammable air, the *gas flammæum* of VAN HELMONT, was considered as an aerial fluid, susceptible of inflammation by itself.

But we now know, that inflammable gas concentrated will not burn at all; but on the contrary extinguishes flame. Mr. CAVENDISH was the first who set this matter in a proper light. He discovered, by experiments, that a mixture of a small quantity of common air with a great proportion of inflammable air, as, for instance, two parts of common air with eight parts of inflammable air, caught fire without noise, and consumed gradually; but that three parts of inflammable air and seven parts of respirable air exploded with a very loud sound.

It was Dr. PRIESTLEY's important discovery which suggested to me the theory I intend to lay before the Royal Society.

This acute philosopher found, that, if instead of common air, dephlogisticated air is mixed with a due proportion of inflammable air, the explosion is considerably louder.

The principal ingredients of gunpowder, and those to which it owes its force, are nitre and charcoal; for these two ingredients, well mixed together, constitute gunpowder at least equal if not superior in strength to common gunpowder (as I found by experience, and may be

seen in the Memoire of Count SALUCE, inserted in the *Melanges de Philosophie et de Mathematiques de l'Acad. Royale de Turin*). The sulphur seems to serve only for the purpose of setting fire to the mass with a less degree of heat.

Nitre yields by heat a surprizing quantity of pure dephlogistified air. Charcoal yields by heat a considerable quantity of inflammable air. The fire employed to inflame the gunpowder extricates these two airs, and sets fire to them at the same instant of their extrication. Thus the difference between the inflammation of gunpowder and that of a mixture of inflammable air with dephlogistified air in an ordinary air pistol (as these last are now contrived for a philosophical amusement) seems to be, that the compound of the two airs in the air pistol takes fire, when already extricated and existing in a space without compression or condensation; that is to say, when they are in no condition of exerting a much greater elasticity than what they acquired by the heat generated in the moment of their explosion; which heat can only expand them to four times their former bulk, according to Mr. ROBINS: whereas in gunpowder the two airs, existing in a solid state before their extrication, and occupying, according to Mr. ROBINS, about $\frac{1}{250}$ of the space they take up after they are set loose, but most probably

bably even less than $\frac{1}{500}$ (as will be seen by and by), are extricated all at once, when confined (as in fire arms) in a space $\frac{1}{250}$ or rather $\frac{1}{500}$ times less than they can occupy when reduced to the temperature of the common atmosphere, and of consequence 2000 less than they can occupy when heated in the moment of inflammation; so that the difference of the explosive force in the inflammation of the two compounds can be no less than as 4 is to 2000.

It must be here remembered, that air being a very compressible body, a moderate resistance acting against its rarefaction easily overcomes the force of its expansion, when this expansion or rarefaction does not amount to more than four times its bulk (that such a power ought not to be very great, we know by the force employed in ordinary wind guns and condensing machines) whereas no condensing machine has yet been contrived by which air could be condensed to any thing approaching the state of condensation of this fluid as it exists in the substance of nitre.

It might be here objected, that air compressed to one tenth in a wind gun possesses a power not much short of gunpowder, though only $\frac{1}{18}$ or $\frac{1}{20}$ of it is let loose at a time; and that thus, inflammable air, though expanded only four times in the moment of inflammation, may exert a force approaching that of the wind gun, the whole

mass of the charge being employed in one and the same explosion. This comparison is very inadequate; for in the case of a wind gun the air compressed to $\frac{1}{10}$ is ready to exert all the force of elasticity existing in the whole mass, and may therefore be compared to a strong spring forcibly bent. But the inflammable air is far from exerting the force of expansion and elasticity through its whole mass at the same instant: for the inflammation is propagated through it successively, beginning where the electrical spark kindles it, and reaching gradually farther till the whole is consumed. Now as I have demonstrated, that inflammable air is reduced to more than half its bulk by inflammation, it must follow, that that portion of it which is consumed the first by the inflammation, leaving more room by its diminution, diminishes in proportion the propelling powers of what remains still to be inflamed.

The very great difference between the explosive force of the two compounds is illustrated by what happens after their inflammation. The compound of inflammable with common or dephlogisticated air, is very much reduced in bulk after inflammation. I found this by the following experiment: I fired a brass inflammable air pistol (made by Mr. NAIRNE according to my directions) which had a piston in the cylinder, by which a proper quantity

quantity of respirable and inflammable air was drawn in. I had rammed into the barrel, adapted to it, a leaden bullet wrapped up in a piece of leather so strongly that I did not expect the resistance could be overcome by the explosion. I fired it by an electrical spark; the inflammation took place, the pistol grew hot, the ball was not propelled, and the piston was driven more than half way down the cylinder by the pressure of the atmosphere acting upon it when the explosive air was consumed by the inflammation.

The case is quite different in the firing of gunpowder, as there remains after its inflammation a mass of air which occupies about 250 times the former bulk, according to Mr. ROBINS.

As, in the foregoing experiment, the compound of inflammable and common air was reduced above the half of its former bulk, it seems more than probable, that the quantity of dephlogisticated and inflammable air extricated in the firing of gunpowder must also undergo a similar diminution by its inflammation; so, that when there remains a mass of air, 250 times the bulk of the gunpowder, the quantity of air extricated from the powder must have been in reality not less than 500 times the bulk of the powder, which agrees nearly with the calculation of Mr. JOHN BERNOULLI. Let us now see

See how far this computation agrees with the analysis of gunpowder. Abbé FONTANA, so advantageously known by his important discoveries in natural philosophy, more especially by those he has made on the various kinds of air, favoured me with the following result of his experiments. An ounce of nitre, exposed to a great degree of heat for the purpose of extracting its air in the usual way, yielded about 800 cubic inches of dephlogisticated air. An ounce of charcoal, treated in the same way, gave about 150 cubic inches of air, partly fixed, partly inflammable, mixed with some common air.

Let us now calculate (without, however, being too scrupulous about the accuracy of the result) what quantity of elastic permanent fluid a cubic inch of solid gunpowder will give in the moment of deflagration: a cubic inch of solid gunpowder contains in weight 442 grains (which is 38 grains short of an ounce Troy weight) of which $331\frac{1}{2}$ grains is nitre, $55\frac{1}{4}$ charcoal, and as much sulphur (supposing the proportion of the ingredients of the powder to be six parts of nitre to one of charcoal and one of sulphur); $331\frac{1}{2}$ grains of nitre will give about 552 cubic inches of dephlogisticated air; $55\frac{1}{4}$ grains of charcoal will produce about 17 cubic inches of air, chiefly inflammable, according to the calculation of Abbé FONTANA.

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By this calculation, which will, perhaps, be found more accurate than the former, one cubic inch of solid gunpowder will yield above 569 cubic inches of permanent elastic fluid: I say, above 569 cubic inches, for I do not put into the account the elastic fluid which is generated by the sulphur, nor that which charcoal, consumed by the inflammation of the gunpowder, yields above the quantity mentioned, which it gives when heated in a glass vessel, by which it is by no means consumed, an ounce losing by this operation only 60 grains of its weight.

As this elastic fluid will increase to four times its bulk, it follows, that one cubic inch of solid gunpowder will extricate in the moment of explosion above 2276 cubic inches of elastic air. Which computation is not far from the result of my former calculation, and that of Mr. BERNOULLI.

An accurate calculation of the expansion of gunpowder would be a very difficult undertaking. The expansion of the moisture always contained in gunpowder, however dry, may also contribute its share towards the amazing powers of this ingredient. Nitre contains from its nature a great share of water, which is necessary for the crystallization of it, and charcoal is always found to contain it. We know,
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that very hot vapour is capable of occupying almost 2000 times the space it did in the state of cold water.

The generation of dephlogisticated and inflammable air by the inflammation of gunpowder is the reason why this ingredient is almost the only one known, which does not want a free access of common air to be consumed by fire; and therefore it may be said to feed, as it were, upon its own air.

This theory of gunpowder induces me to venture a new one of the *pulvis fulminans*, which consists of three parts of nitre, of two of fixed alkaline salt, and of one of sulphur. This powder much surpasses the force of gunpowder in exploding, with a very loud report, in the open air when it is heated to a certain degree. It is commonly said, that in the heating of this powder the sulphur joins with the alkaline salt and constitutes an *hepar sulphuris*, which rising up in bubbles confines the air contained in them, which air at last becomes so powerfully expanded that it overcomes and breaks through the resistance of the coercive bubbles of the *hepar sulphuris*, with all the force of its elasticity; which sudden emersion must naturally occasion a proportional sound. But I think, that the nitre contained in this powder, being heated, yields its dephlogisticated air when the melting sulphur yields inflammable air; at the same time the sulphur constitutes
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with the alkaline salt, an *hepar sulphuris*, which rising in tough bubbles confines this explosive air generated. At length, however, the increasing heat, which sets fire to the sulphur, sets this explosive air on fire also; which then following its own nature explodes with so much the more force from its having been entangled and confined within the bubbles of the *hepar sulphuris*.

After what has already been said, it will not be difficult to explain, why a single spark of fire propagates the combustion with great rapidity through the whole mass of gunpowder, however great. If we put a single grain of gunpowder upon a red-hot iron, we see the particles of red-hot charcoal projected with great rapidity in every direction by the forcible explosion of the two airs extricated in the manner before explained. Thus, if one or more grains, among a heap of others, are set fire to, the particles of red-hot charcoal being driven with great violence against the surrounding grains, communicate their heat to all the particles of charcoal they hit, which particles, by heating the particles of nitre in close contact with them, extricate their dephlogisticated air at the same time that the charcoal yields its inflammable air; in consequence of which a more powerful explosion happens. This secondary explosion projects with a much greater force, the particles of charcoal surrounded by the explosive

flame of the two airs; and thus the conflagration spreads with a very great velocity through the whole mass, though always by succession. The quickness of this propagation of fire depends in a great measure upon the intervals or interstices which remain among the grains of gunpowder, through which the particles of heated charcoal are driven in every direction, together with the flame of the two airs. Hence gunpowder reduced into impalpable powder, and rammed into a squib, does not inflame with an explosion, but burns slowly farther and farther till the combustion reaches the extremity of the squib, where it meets a mass of gunpowder in grains, when immediately a loud explosion issues, by which the squib is shattered into rags. Hence the size of the grains of gunpowder must be proportionate to the size of the fire arms to which it is destined, the greatest fire arms requiring in general grains of the largest size.

If this wonderful and awful ingredient had not been discovered by accident, could the secret have escaped a long while the penetration of our modern philosophers, who have found out the way of combining the air of the two constituents after they had extricated them, without any regard to the known properties of gunpowder? Nothing more was to be done than combining the two sub-

stances instead of combining the two airs first separated from them.

A P P E N D I X.

IN the foregoing paper I attempted to give a comparative view of the explosive force of gunpowder and inflammable explosive air, which latter I had found to be so far short of the explosive force of gunpowder as not to conceive any well grounded hope that it could ever become a substitute to this ingredient.

At that time I had not yet tried the effect of very pure dephlogisticated air combined with that inflammable air, into which I had found that vitriolic æther is changed in an instant.

I must acknowledge, that I had but small expectations from the force of these two airs combined; for as I had always observed, that æther air combined with common air is less brisk in taking fire, and less powerful in exploding, than inflammable air extracted from the vitriolic or marine acid, I thought that the same æther air combined with very pure dephlogisticated air would also

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be less powerful than common inflammable air from metals. But how far experience contradicted this theoretical analogy will be seen in the following lines.

Abbé FONTANA was so good as to assist me in this pursuit. Having produced a good quantity of pure dephlogisticated air from red precipitate by heat, we first filled a strong two-ounce phial (the orifice of which was so wide that it could scarce be covered with the thumb, so that the bottle was almost cylindrical) with this air, in the usual manner, by filling it first with water, inverting it, and letting the air rise in it; which being done, we dropped one drop of æther (in which a small quantity of camphire was dissolved) into it, and shut it immediately with the thumb. After having given it some concussions, the orifice was applied to the flame of a candle, by withdrawing the thumb when the orifice was close to the flame: the air instantly took fire, and exploded with such a strong report, that, if the phial had not been very stout, it would most probably have been shattered into pieces, notwithstanding its wide orifice. We repeated the same experiment with the same success.

I was the more astonished at the uncommon loud report (considering the wide orifice of the phial), because, having often tried æther air in the same way with common air, I never found it explode with any
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considerable degree of force; and therefore I found it necessary, in order to procure a loud report, to kindle it by an electrical spark directed through the pistol, when its orifice was shut up by a cork, the resistance of which was the chief cause of the report.

This wonderful effect in an open vessel could not fail of giving me a good expectation of a very powerful effect, if this compound air was shut up in an air pistol by a cork squeezed into its orifice. As it had been now kindled twice by the flame of a candle, I wanted to kindle it by the same means in an air pistol; for this purpose we drilled a small hole in the side of the pistol, which was made of tin, and contained about nine cubic inches of space. We filled it with dephlogisticated air in the same manner as we had filled the phial by means of water; and after having poured into it one drop of æther by means of a glass tube (in the manner above described), we shut the orifice by thrusting a cork into it, and kept a finger applied to the touch-hole which was drilled in the side of the pistol. To avoid accidents if the pistol should burst, we thought it prudent to squeeze the cork very gently into the orifice, so that the resistance should be very moderate. Abbé FONTANA wrapped a towel round the pistol for security's sake, leaving only the touch-hole uncovered; which being brought near the flame
of

of a wax taper, the air instantly took fire, and exploded with such a strong report, that his hearing, as well as mine, was much hurt by it. The cork, which was a very sound one, flew to pieces against the wall; and the Abbé felt such a considerable shock in his hands, that he did not think it safe to repeat the experiment, unless a stronger pistol could be procured.

Encouraged by such uncommon and unexpected effects, I went immediately to Mr. NAIRNE to inquire, whether he still had in his possession a strong brass air pistol, which he had made last summer according to my direction? I was lucky enough to find it: nothing was to be done to it but to drill a touch-hole in the left side of it, in order to kindle it by a flame if required. This touch-hole was to be shut up by a brass male screw fitted exactly to it, when the pistol was intended to be fired by an electrical spark.

The air box of this pistol was a cylinder four inches long and two inches in diameter. The fore part of the air box to which the pistol barrel fitted to receive a leaden ball or a cork, was fixed, had a broad shoulder, which was fastened to the body of the air box by six strong brass screws, which never had been loosened by former explosions. A leaden bullet, wrapped up in leather, was forcibly rammed into the pistol barrel as far as the screw, which joins

joins the barrel with the air box (as may be seen in the figure). The pistol was filled with pure dephlogisticated air (which was drawn-in by the piston from an elastic gum bottle), and one drop of æther being poured into it, the air within was kindled by an electrical spark directed through it. The air took fire: the explosion was as loud as that of a common musket, and the force so great, that the whole fore part of the air box with the pistol barrel flew off, all the fix screws were broke, and the strong and tough metal of which they were made was rent. Three strong brass screws, by which the bottom of the air box was fixed to the wooden handle, were loosened, and the whole frame of the pistol was out of order. The substance of the air barrel, where it was tore, was of the thickness of about a half crown piece.

Being now convinced, that though inflammable air from metals with dephlogisticated or common air, is far inferior to the force of gunpowder, the explosive force of the compound of dephlogisticated and æther air approaches it much nearer, I thought it worth while to fit the pistol up in such a manner as to be out of all danger of bursting. For this purpose I desired Mr. NAIRNE to adapt, and solder to the fore part of the air box, a hollow cone of brass, the extremity of which should terminate in the gun barrel.

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As the piston could not reach to the extremity of this conical hollow (which consequently must be always filled with common air), I desired him to fix to the piston an ivory cone, through which the two wires would pass to meet one another at the surface of the cone, leaving an interstice between them of about one line, through which the electrical spark should leap and set fire to the air. This ivory cone shutting up exactly the whole cavity of the air box, no air could come into it but what was drawn in by the piston.

The pistol thus fitted up answered tolerably well. The figure joined to it will serve to give a better idea of the whole contrivance than could be well explained by words. The scale is in the proportion of one third of the real size.

The cone, instead of ivory, may be made of solid glass, which is a better non-conductor than ivory. The canals in the ivory, through which the two wires pass, may be made wide enough to contain a glass tube, through which the wires pass; or to be filled with a non-conducting cement, as sealing wax, for the same purpose. The cone may even be made of brass, provided two glass tubes are lodged in it, to give a passage to the two wires.

I kindle this pistol sometimes by putting in the touch-hole a little bit of a cotton thread soaked in moist gun-powder

powder and dried afterwards; or a bit of those paper matches which the Chinese put into those little squibs, which go by the name of *India crackers*. I sometimes kindle it by holding the flame of a candle or a burning paper to the touch-hole. In this case it is to be observed, that the touch-hole must be kept upwards, if the pistol is loaded with inflammable air from metals, because this air being lighter than common air, will rise out of the hole and meet the flame. The contrary must be done when æther air is employed, it being heavier than common air, and thus disposed to descend and fall upon the flame kept under it.

To fill this pistol with any air, I commonly first fill an elastic gum bottle with it, the orifice of which is just big enough to receive that part of the gun barrel which is fixed to the air box: thus, by squeezing between my feet the elastic gum bottle, I draw in at the same time the air by drawing up the piston. A bladder is also very fit for this purpose, and has the advantage above an elastic gum bottle in not requiring to be squeezed to draw the air out of it.

Inflammable air from metals will rise in the pistol of itself, when its orifice is kept upon the bottle containing it.

If the pistol is destined to be always kindled by the flame of a candle or a match, as I have described, it would be better to have no piston to it, as it may then be filled by the means of water, and the explosive force will be so much the greater, as some of the flame makes easily its way over the leather of the piston, and rushes out backward, which, I find, is often the case, if the bullet is rammed in the barrel somewhat too tightly.

It would, perhaps, not be an easy undertaking to give a satisfactory reason, why a drop of æther communicates to dephlogisticated air a much stronger explosive force than common inflammable air from metals. May it not be said, that common inflammable air from metals, having only about one fifth of the specific gravity of the dephlogisticated air, the two fluids do not penetrate one another so readily and so intimately as the compound of dephlogisticated and æther air, which are both nearly of the same specific gravity, each being somewhat heavier than common air? for it seems not improbable, that the swiftness with which the flame is propagated through the mass of this compound air, depends partly on the intimate mixture of the phlogiston with the dephlogisticated air. Might not this phenomenon be ascribed to the greater bulk of inflammable air from metals compared with the small compass which one single drop

drop of æther occupies, which last ingredient, when pure, seems to be an essence of the inflammable principle of the spirit of wine, a pure phlogiston concentrated in the form of a liquid? Indeed the inflammable air from metals seems to be rather a compound of phlogiston and some kind of elastic permanent fluid than a pure inflammable fluid; for this air, after having lost all its inflammability, by being kept a long while upon water, occupies still a considerable space, and is then become phlogisticated air; that is to say, such an air as is not to be diminished by nitrous air, or to be inflamed.

Though I have no reason to alter my former assertion, that the force of gunpowder is proportionable to the sudden extrication of a great quantity of the elastic fluid generated in the moment of conflagration, and the expansion of this fluid by heat, communicated to it in the same moment of its extrication; and that the force of inflammable explosive air can only be proportionable to the sudden expansion by heat in the moment of the inflammation (for no new extrication here takes place); yet I did not consider enough in the account the suddenness of this expansion, which may make a considerable difference in the force of the explosion. And indeed the abovementioned experiments seem to demonstrate, that the inflammation of the compound of pure dephlo-

gified and æther air spreads with such a velocity through the whole mass as to be almost instantaneous.

It is well known, that mechanical power chiefly depends upon the velocity with which a body is endowed in the instant of exerting it; or that the *momentum*, or force of a body, must be computed by multiplying the quantity of matter into the velocity with which it moves. Thus, if this new compound of dephlogified and æther air expands with ten times greater velocity than any other inflammable explosive air, its force will be about ten times greater.

As it seems to be probable, from what is already said that this compound of explosive air may be put to more uses than that of an amusing experiment, I think it worth while for men engaged in this branch of natural philosophy to look out for a method of producing at pleasure any quantity of dephlogified air required. Considering the rapid progress which is daily made on the important subject of air, I cannot but flatter myself, that this great discovery is not far off. The benefit which would arise from such a discovery for animal life must encourage every philosopher to pursue this object. Indeed, if we consider that nitre contains this wonderful aerial fluid in a most concentrated state, and that the nitrous acid seems to be nothing else but this beneficial
fluid

fluid combined with phlogiston, which seems to be imbibed by the vegetable alkali, when the acid is expelled by heat in the form of this air; that this beneficial aerial fluid exists also, in a most concentrated state, in bodies almost every where to be found, as are calces of metal, principally that of iron; that common water contains it in great abundance, so that the light and warmth of the sun extracts it to one fifteenth of the bulk of the water, as Dr. PRIESTLEY found, that even the mass of our atmosphere is nothing else but this very air soiled with impurities. If we consider, I say, all this, is it not reasonable to hope, that we are near the important instant when this salubrious aerial fluid will be procured for many useful purposes in a sufficient quantity, either by the discovery of a ready way to let loose this air from the bodies in which it is as it were imprisoned, or by filtering or purifying common air from its impurities?

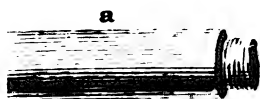
EXPLANATION OF THE FIGURE.

- A a the pistol barrel.
- B the large cylinder or air box.
- b the place where the pistol barrel unscrews from the air box.
- c the pistol handle.

D the

- D** the handle fixed to the piston at *bb*, which is square to prevent the pistons turning round.
- E** the hole in the side of the air box.
- e** the screw to stop the said hole.
- F** a piece of brass with a female screw, which was fixed with three strong screws to the wooden handle.
- f** a screw at the end of the air box which screwed into the piece **F**.
- G** a piece of ivory fixed to the piston.
- K** the piston, with the piece of conical ivory fixed to it.
- L** the termination of the ivory cone which filled up the small end of the air box.
- M** the perforation in the wooden handle, through which the brass handle **D** passes.
- N** a brass ball at the end of a wire, which passes down the canal **R** in the ivory cone.
- O** another piece of wire passing down the other canal, canals are filled with non-conducting substance.
- P** an interstice between the two wires of about one line, through which the electrical spark, when given on the ball **N**, passes and sets fire to the inflammable air in the air box.





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XXVH. *The Description of Two new Micrometers.* By
Mr. Ramsden, Optician; communicated by Joseph
Banks, Esq. P. R. S.

Read March 25, 1779.

WHEN I offer the description of a new micrometer to the learned members of the Royal Society, I do not flatter myself that mere novelty will entitle it to their patronage. Sensible how much the theory of astronomy is limited by the imperfection of instruments, I always incline to improve rather than invent, except when repeated examinations convince me, that the imperfections arise from defect in principle as well as in the construction.

In considering micrometers, when an observer finds, that with the micrometer, which depends on moveable parallel wires, he cannot measure any diameter of a planet, except that which is at right angles to the direction of its apparent motion, he cannot withhold his preference to that construction which measures the angle by the separation of the images. It appeared therefore to,

to me a matter of some importance, to investigate the causes of the uncertainty which has been found in the observations made with the micrometer with a divided object-glass. The result of my examination convinced me, that were it possible to execute the construction of that micrometer, with the degree of accuracy required, it must still be subject to inaccuracy from its principle.

By the position of the micrometer every error of its glass is magnified by the telescope; and if each surface of the micrometer glass has not, in every part, precisely the same radius (which opticians must allow to be exceedingly difficult) there will be a considerable error in the angle to be measured, and the eye applied to the different parts of the pencil will, without moving the micrometer, see the images of the object in the telescope fluctuating, sometimes appearing to overlap, and sometimes to separate from each other.

But supposing the glass itself to be perfect in its substance and in its curvature, there will yet remain imperfections which arise from its principle. A micrometer glass applied to a telescope causes a very considerable aberration. If the focus of the glass is positive, the extreme aberration will be within the geometrical focus; if negative, it will be beyond it: and the aberration not only
affects

affects the distinctness of the image, but also the angle measured by the micrometer.

At the time I took up this subject, the divided object-glass micrometer was the only one which measured angles by the separation of two images. Since that time, a very ingenious application of the prism to this purpose has been invented by the rev. Dr. MASKELYNE, Astronomer Royal; and although experience has not yet ascertained the extent of its merit, it will always deserve great consideration from its ingenuity; but the more I considered the subject, I became more fully convinced, that the principle of reflection applied to micrometers would have great advantages over those hitherto constructed on the principle of refraction; and the catoptric micrometer I have the honour to describe, besides the advantage it derives from the principle of reflection, of not being disturbed by the heterogeneity of light, avoids every defect of other micrometers, and can have no aberration, nor any defect which arises from the imperfection of materials, or of execution, as the extreme simplicity of its construction requires no additional mirrors or glasses to those required for the telescope: and the separation of the image being effected by the inclination of the two specula, and not depending on the focus of any lens or mirror,

ror, any alteration in the eye of an observer cannot affect the angle measured.

It has, peculiar to itself, the advantages of an adjustment to make the images coincide in a direction perpendicular to that of their motion; and also of measuring the diameter of a planet on both sides the zero, which will appear no inconsiderable advantage to observers who know how much easier it is to ascertain the contact of the external edges of two images than their perfect coincidence. A short explanation of the annexed drawings will make the construction and the properties of this micrometer obvious.

I divided the small speculum of a reflecting telescope, of CASSEGRAIN's construction, into two equal parts, by a plane across its center; and by inclining the halves of the speculum to each other on an axis at right angles to the plane that separated them, I obtained two distinct images. The satisfaction I received on the first trial was checked by the apparent impossibility of reducing this principle to practice. The angular separation of the two images in this case being half the angular inclination of the two specula, it required an index of an unmanageable length, to allow the quantity of one second of a degree to become visible. Some time afterwards, on revising the principle, I considered, that if both the halves of the mir-

ror turned on their center of curvature, there could be no alteration in their relative inclination to each other from their motion on this center; and that any extent of scale might be obtained, by fixing the center of motion at a proportional distance from the common center of curvature. This will be better understood from the annexed drawing.

A (fig. 1.) represents the small speculum divided into two equal parts; one of which is fixed on the end of the arm B; the other end of the arm is fixed on a steel axis *x*, which crosses the end of the telescope *c*. The other half of the mirror A is fixed on the arm D, which arm at the other end terminates in a socket *y*, that turns on the axis, *x*; both arms are prevented bending by the braces *aa*. *g* represents a double screw, having one part *e* cut into double the number of threads in an inch to that of the part *g*: the part *e* having 100 threads in one inch, and the part *g* 50 only. The screw *e* works in a nut *F* in the side of the telescope, while the part *g* turns in a nut *H*, which is attached to the arm B; the ends of the arms B and D, to which the mirrors are fixed, are separated from each other by the point of the double screw pressing against the stud *b*, fixed to the arm D, and turning in the nut *H* on the arm B. The two arms B and D are pressed against the direction of the double screw *eg* by

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a spiral

a spiral spring within the part *n*, by which means all shake or play in the nut *n*, on which the measure depends, is intirely prevented.

From the difference of the threads on the screw at *e* and *g* it is evident, that the progreffive motion of the screw through the nut will be half the distance of the separation of the two halves of the mirror, and consequently the half mirrors will be moved equally in contrary directions from the axis of the telescope *c*.

The wheel *v* fixed on the end of the double screw has its circumference divided into 100 equal parts, and numbered at every fifth division with 5, 10, &c. to 100, and the index *I* shews the motion of the screw with the wheel round its axis, while the number of revolutions of the screw is shewn by the divisions on the same index. The steel screw *R* may be turned by the key *s*, and serves to incline the small mirror at right angles to the direction of its motion. By turning the finger head *τ* (fig. 2.) the eye tube *p* is brought nearer or farther from the small mirror, to adjust the telescope to distinct vision; and the telescope itself hath a motion round its axis for the convenience of measuring the diameter of a planet in any direction. The inclination of the diameter measured with the horizon is shewn in degrees and minutes by a level and vernier on a graduated circle, at the breech of the telescope.

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The method of adjusting and using the catoptric micrometer is too obvious to require any explanation: it is only necessary to observe that, besides the table for reducing the revolutions and parts of the screw to minutes, seconds, &c. it may require a table for correcting a very small error which arises from the excentric motion of the half mirrors. By this motion their centers of curvature will (when the angle to be measured is large) approach a little towards the large mirror; the equation for this purpose in small angles is insensible, but when angles to be measured exceed ten minutes, it should not be neglected. Or, the angle measured may be corrected by diminishing it in the proportion the versed sine of the angle measured, supposing the excentricity radius, bears to the focal length of the small mirror.

The telescope to which the catoptric micrometer is applied is of the CASSEGRAIN construction. The great speculum is about twenty-two inches focus, and bears an aperture of $5\frac{1}{16}$ inches, which is considerably larger than those of the same focal length are generally made: indeed, the apparent utility of this micrometer makes me wish to see the reflecting telescope meet with further improvements. I believe it would more tend to the advancement of the art of working mirrors, if writers on this subject, instead of giving us their methods of working ima-

imaginary parabolas, would demonstrate the properties of curves for mirrors which, placed in a telescope, will shew images of objects perfectly free from aberration; or, what will yet be more useful in practice, of what forms specula might be made, that the aberration caused by one mirror may be corrected by that of the other. If mathematicians assume *data* which really exist, they must see, that when the two specula of a reflecting telescope are parabolas, they cause a very considerable aberration, which is negative, that is to say, the focus of the extreme rays is longer than those of the middle ones. If the large speculum is a parabola, the small one ought to be an ellipse; but when the small speculum is spherical, which is generally the case in practice, if concave, the figure of the large speculum ought to be an hyperbola; if convex, the large speculum ought to be an ellipse, to free the telescope from aberration.

This will be easier understood by attending to the positions of the first and second images; when a curve is of such form that lines drawn from each image, and meeting in any part of the curve, make equal angles with the tangent to the curve at that point, it is evident, that such curve will be free from aberration.

This is the property of a circle when the radiant and image are in the same place; but when they recede
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from each other, of an ellipse, of such form that the radiant and image are in the two *foci*, till one distance becoming infinite the ellipse changes into a parabola, and to an hyperbola when the focus is negative; that is to say, when reflected rays diverge, and the focus is on the opposite side of the mirror.

These principles made me prefer CASSEGRAIN'S construction of the reflecting telescope to either the Gregorian or Newtonian. In the former, errors caused by one speculum are diminished by those in the other.

From a property of the reflecting telescope (which has not been attended to) that the apertures of the two specula are to each other very nearly in the proportion of their focal lengths, it follows, that their aberrations will be to each other in the same proportion, and these aberrations are in the same direction, if the two specula are both concave; or in contrary directions, if one speculum is concave, and the other convex.

In the Gregorian construction, both specula being concave, the aberration at the second image will be the sum of the aberrations of the two mirrors; but in the CASSEGRAIN construction, one mirror being concave, and the other convex, the aberration at the second image will be the difference between their aberrations. By assuming such proportions for the *foci* of the specula as are gene-

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rally used in the reflecting telescope, which is about as 1 to 4, the aberration in the CASSEGRAIN construction will be to that in the GREGORIAN as 3 to 5.

I have mentioned these circumstances in hopes of recommending the demonstration of curves suited to the purposes of optics to the attention of mathematicians, which would be of great use to artists.

I shall conclude this paper with the description of a new micrometer suited to the principle of refraction; being sensible that both principles have their peculiar advantages. Though the former part of this paper proves my partiality to the principle of reflection applied to micrometers, yet the very favourable opinion I have of the refracting telescope made me attentively consider some means of applying a micrometer to it, which might obviate the errors complained of in the former part of this paper.

The application of any lens or medium between the object glass and its focus must inevitably destroy the distinctness of the image; I therefore have employed for the micrometer glass one of the eye glasses requisite in the common construction of the telescope; but if it should be found necessary to apply an additional eye glass for the convenience of enlarging the scale, I am able thereby to correct

rect both the colours and spherical aberration of the first eye glass.

This micrometer is applied to the erect eye tube of a refracting telescope, and is placed in the conjugate focus of the first eye glass: hence arises its great superiority to the object glass micrometer. It has been before observed, that if a micrometer is applied at the object glass, the imperfections of its glass are magnified by the whole power of the telescope; but in *this* position, the image being considerably magnified before it comes to the micrometer, any imperfection in its glass will be magnified only by the remaining eye glasses, which in any telescope seldom exceeds five or six times.

By this position the size of the micrometer glass will not be the $\frac{1}{100}$ th part of the area which would be required if it was placed at the object glass; and, notwithstanding this great disproportion of size, which is of great moment to the practical optician, the same extent of scale is preserved, and the images are uniformly bright in every part of the field of the telescope.

Fig. 4th, represents the glasses of a refracting telescope; *xy* the principal pencil of rays from the object glass *o*; *tt* and *uu* the axis of two oblique pencils; *a* the first eye glass; *m* its conjugate focus, or the place of the micrometer; *b* the second eye glass, *c* the third, and *d* the

fourth, or that which is nearest the eye. Let p be the diameter of the object glass, e the diameter of a pencil at m , and f the diameter of the pencil at the eye; it is evident, that the axis of the pencils from every part of the image will cross each other at the point m , and e , the width of the micrometer glass, is to p the diameter of the object glass as ma is to go , which is the proportion of the magnifying power at the point m , and the error caused by an imperfection in the micrometer glass placed at m will be to the error, had the micrometer been at o , as m is to p .

Fig. 3d, represents the micrometer; A a convex or concave lens divided into two equal parts by a plane across its center; one of these semi-lenses is fixed in a frame B, and the other in the frame E, which two frames slide on a plate H, and are pressed against it by thin plates aa : the frames B and E are moved in contrary directions by turning the button D; L is a scale of equal parts on the frame B; it is numbered from each end towards the middle with 10, 20, &c. There are two verniers on the frame E, one at M, and the other at N, for the convenience of measuring the diameter of a planet, &c. on both sides the zero. The first division on both these verniers coincides at the same time with the two zero's on the scale L, and,



and, if the frame is moved towards the right, the relative motion of the two frames is shewn on the scale *L* by the vernier *M*; but if the frame *B* be moved towards the left, the relative motion is shewn by the vernier *N*.

This micrometer has a motion round the axis of vision, for the conveniency of measuring the diameter of a planet, &c. in any direction, by turning an endless screw *F*, and the inclination of the diameter measured with the horizon is shewn on the circle *g* by a vernier on the plate *v*. The telescope may be adjusted to distinct vision by means of an adjusting screw, which moves the whole eye tube with the micrometer nearer or farther from the object glass, as telescopes are generally made; or the same effect may be produced in a better manner, without moving the micrometer, by sliding the part of the eye tube *m* on the part *n*, by help of a screw or pinion. The micrometer is made to take off occasionally from the eye tube, that the telescope may be used without it.



XXVHI. *Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé Fontana, Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of Tuscany, to Joseph Priestley, LL.D. F. R. S.*

Read April 16, 1779.

I TAKE the liberty of sending you an account of some experiments which I made at Paris in the years 1777 and 1778 on the air extracted from various kinds of waters: some of the principal of which I thought proper to transcribe, that you might make use of them if you think that they are at all useful, and likewise lay them before the Royal Society.

I have extracted the air from the water of a well by means of common fire. The water was then made to boil in a large matrafs of tin, which had a long tube of the same metal, which being bent into two different directions was with its extremity immersed in a tub of cold water. The matrafs and its tube were intirely filled

filled with water: the air which came out of it was received into three different vessels. The air of the first vessel, by being shaken in water, was diminished a little; the air of the second was diminished of half its bulk, or rather more; and the air of the third vessel was diminished exceedingly. The *residuum*s of air that remained unabsorbed were more or less phlogisticated.

Another time I obtained almost entirely fixed air, excepting a little which remained unabsorbed by water, and was partly phlogisticated.

A third time the air of the water of a well, obtained as above, was made to pass through mercury into a tube anointed with oil of tartar, and it occasioned a crystallization just like that which the purest fixed air is used to do.

A fourth time I impregnated with this air a quantity of common water, which absorbed its own bulk of it, and became by these means acidulous, exactly like water with the purest fixed air. This water turned the tincture of turnsole red, and precipitated the lime in lime water. Another time a light was successively extinguished, and a bird died instantly in this air.

The water of the river Seine, filtrated through sand, as it is drunk at Paris, was treated in the same manner as the water of the well. The air extracted from that water was half absorbed by water, when shaken in it; the remainder,

remainder, when treated with the taste of nitrous air, gave II-4, II+1^(a); when the common atmospheric air treated with the same nitrous air gave II-4, II+8. It was therefore sensibly better than the atmospheric air, which, during three years of experiments made at Paris, I have constantly found to be inferior to the air of the Seine water, extracted as above.

Having repeated the experiment, I received the air into two different receivers. The first of which, by being shaken in water, was diminished in the proportion of ten to seven; and by the test of nitrous air gave II-14, II+1, III+1, when the common air gave II-12, II+6, III+6.

The second quantity of air was diminished in the proportion of three to one; and when examined by the test of nitrous air gave II±0, III±0. From whence it may be concluded, that the first air was better than the atmospheric air; whereas the second was worse, and mixed with much fixed air.

Being in doubt whether the tin vessel employed in the experiment above mentioned might not alter the nature of the air, &c. I made use of glass vessels. Having therefore filled one of these vessels, having a long neck bent in two directions, with the Seine water, I

(a) See p. 343. for an explanation of this measure.

obtained some air which seemed not sensibly diminished when shaken in water. Having introduced one measure and 37 parts of this air into the tube used to try the diminutions, it gave with the nitrous air $I+19$, $I+48$, when the same quantity of nitrous and atmospheric air gave $II+26$, $II+6$: it is therefore certain, that the air extracted from Seine water is purer than common air.

Another time I extracted, in the same manner, and from the same water, the air; one measure and 24 parts of which being introduced into the tube, &c. and shaken, was reduced to one measure – 31 parts, that is, one fifth of it was absorbed. Treated with the nitrous air it gave $I-4$, when equal measures of common and nitrous air gave $I\pm 0$: it was therefore better than common air.

A third time I extracted the air, in the manner above mentioned, from the water of the river Seine, contained in three matrasses; this air was about one twenty-eighth of the bulk of the water, and it gave with the test of nitrous air $II-14$, $II-9$, $III-9$; when the common air mixed with nitrous air, as usual, gave $II-14$, $II+8$, $III+8$. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glass vessels, is much better than common air, or than the air which is extracted from the same water when boiled in tin vessels.

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Another time I filled a glass retort, which had a long and doubly bent neck, with Seine water. The water weighed about three pounds. The air that came out of it lost a quarter of its bulk by being shaken in water; and afterwards being tried with nitrous air it gave II-16, II-16, III-16, when common and nitrous air gave II-12, II+12.

This experiment being repeated, the air was diminished of one quarter by being shaken in water. One measure - 16 parts of this air introduced into the measuring tube gave II-32, II-2, when common air and the nitrous gave II-28, II+4.

The water d'Argeuil at Paris is considered as very pure. I filled the tin vessel above mentioned with it, and received the air that came out of it into three vessels. Being shaken in water, the first of them was diminished one fifth; the second, three fourths; and the third $\frac{15}{16}$ by the operation in water. A light burned with a flame, more luminous than in common air, in the first air after it had been shaken in water. This air being tried with the nitrous air gave II-10, II-10, III-10. The second gave II-10, II-17, III-30, when common and nitrous air gave II-2, II+14, III+14. The third air, before it was shaken in water, crystallized with the oil of tartar like fixed air. An equal bulk of it was absorbed by

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water,

water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguished a light several times, and killed an animal instantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewise than that extracted from Seine water, even when this last has been boiled in glass vessels.

The experiments being repeated with the same water of Arqueil, but in glass vessels, the air obtained, after being shaken in water, was much better than that obtained from the same matter, when boiled in vessels of tin.

I have also extracted the air from distilled water in glass vessels, and having shaken one measure – 32 parts of it in water, it was reduced to one measure – 35 parts. With the test of nitrous air it gave 1–6, when equal parts of common and nitrous air gave 1–2, which shewed that it was better than common air.

I extracted the air again in the manner above described; but it was not sensibly diminished when shaken in water. Two measures – 49 parts of it, with the test of nitrous air, gave 1–2, 1+8, when common air, &c. gave 1+1, 1+18: It is therefore better than common air.

I extracted the air from a great quantity of distilled water in the usual manner, and found that it did not sen-

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fibly diminish in water. With nitrous air it gave II-14, II-25, II+25, when common and the same nitrous air gave II-14, II+10, III+10; consequently it was dephlogisticated air, *viz.* purer than the air of the Seine and Arqueil, which are much better than common air.

I had the curiosity to try, whether any difference would arise from boiling distilled water in a matrafs of tin instead of glass vessels; and found, that the first air was diminished one tenth by being shaken in water, and afterwards with the nitrous air gave II-13, II-16, III-18, when common air gave II-12, II+8; which shews that it was dephlogisticated air, but not so good as that extracted from the same water when boiled in glass vessels. The second quantity of air was not sensibly diminished in water, and with nitrous air gave II-13, II-20, III-30; that is, it was more dephlogisticated than the first.

The air extracted from distilled water is to that extracted from the water of the river Seine as 13 to 32 nearly; whence distilled water does not give more air than one sixtieth of its bulk: but as the air extracted from the water of the Seine is half fixed air, it may be concluded, that the quantity of respirable air produced by both kinds of waters is nearly the same, and that they only differ a little in purity. It is however true, that other ex-

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periments.

periments have shewn me that water in general absorbs about twice as much of dephlogisticated as of common air; for which reason, I think, that the respirable air of Seine water is rather less than that of distilled water. Accordingly I have found, that Seine water, after it has been boiled for a long time, absorbs in forty days about one fourteenth of its own bulk of dephlogisticated air, when in the same length of time it does not absorb more than one twenty-eighth of common air. This seems to be an experiment of very great consequence, and is much worth notice; especially as it discovers a new characteristic by which dephlogisticated air may be distinguished from common air; and shews, that water absorbs a greater quantity of those kinds of air, which contain a less quantity of phlogiston.

It must however be observed, that it is impossible to determine exactly the quantity of air that is extracted from vessels filled with water, by means of fire; because a portion of the air is absorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in vessels immersed in quicksilver; but then there are many other inconveniencies to encounter.

It may be almost superfluous to mention, that the above related experiments are very useful in explaining

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the reason why some kinds of water have a peculiar sharp taste more than others; and especially why some of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnsole into a red colour, as I have generally experienced with the well waters at Paris. We may also explain from hence, why some kind of waters can dissolve iron, and keep it in dissolution without deposition; whereas other kinds of water are incapable of doing it, at least do it much less than the purest distilled water. This is soon discovered by boiling the water, which will then deposit the iron which before was dissolved.

It will be sufficient, for the present, to mention, that I have not only extracted from waters the different kinds of air they contained naturally, but have likewise made various experiments upon waters deprived of air, which, being exposed, have again imbibed the atmospherical air, as I hinted above. I have determined the quantity and quality of those airs. In general, I may say, that distilled water, deprived of air, imbibes again an equal quantity of air of the same kind as that it had lost, and that in less than fifty days. Other kinds of water do the same, but with this difference, *viz.* that the air they absorb, after being boiled, is better than that they have lost; and in this particular they come very near to the nature of distilled water itself.

If the waters deprived of air are exposed to common air held in receivers in contact with quicksilver, the air which remains unabsorbed is so much more phlogisticated as a less quantity of it remains in the receiver. This experiment deserves consideration.

By means of pure water, especially distilled water, common air may be changed into dephlogisticated air, that is, into air much more salubrious than the best common air which we breathe; and this, for what I know, is the only means of meliorating common air: for all the artificial methods (great numbers of which I have tried) have proved either useless or noxious, but never such as promised to be of any great utility to human kind.

Though I have long thought of applying those experiments to some use for the purposes of life, the want of time and a proper apparatus has hitherto hindered my doing any thing; I now begin to be in hopes I shall be able to do something. In the mean time I think it of some importance to have it known, that water not only possesses the property of diminishing the noxious part of tainted air, but has also the power, and that in a very high degree, of dephlogisticating common air; which must certainly be one of the methods by which nature keeps the atmosphere in a state constantly fit to support animal life, it being certain, that the water in various circumstances

circumstances must lose either a part or the whole of that air which it hath absorbed from the atmosphere.

It may with some reason be suspected, that in my experiments of extracting the air from water by the action of fire, the air might be considerably altered by the vapour of the water itself. As this difficulty was of some force, I endeavoured to remove it in the following manner. I introduced into a tube, through water, a quantity of common air of known goodness, and I caused the steam of water boiling in a matrafs, from which the air had been previously extracted, to pass through it. The heat of the steam sometimes made the water occupy above five times the space it did when cold; yet the air so treated was not at all altered by it, as appeared by the test of nitrous air. The event of the experiment, although repeated various times, was constantly the same.

I must observe, lastly, that having once caused the air of boiling water to pass into receivers filled with, and standing in, quicksilver, I found that the air was better than usual. I have observed the same thing when I have caused the air to go through distilled water into receivers filled with it: which observation, if the event of the experiment is constantly the same, induces me to believe, that the air loses some of its good properties by going through water not very pure; or, which seems to be rather

rather more probable, that a quantity of air less good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matrafs; from whence this air is debased.

I must not omit to mention a new character of equal importance with that which distinguishes the dephlogisticated from the common air. This new character has been equally unknown, and deserves the attention of philosophers, because at the same time it discovers a new property of the atmospherical air, which I should never have suspected if experience had not offered it to me.

I have found, that common air shaken in water, instead of being diminished is sensibly increased in its bulk. The increased space is in proportion to the time the air is shaken in water, and it begins to be sensible even from the beginning, that is, after a few seconds. This augmentation I have sometimes brought to be one twelfth of the bulk of the air, and even more: it must, however, be confessed, that I met with great variety in the experiments of this kind made at different times. After that the bulk of the air shaken in water is increased to a certain degree, it then begins to decrease continually; and, in proportion to this decrease, the air becomes gradually less good. When the experiment is tried in close vessels, the diminution.

nution cannot be observed; but I shall reserve, for another opportunity, to speak of the laws and causes of those diminutions and augmentations, and the differences observed between common and other kinds of air, and when the experiments are tried in water, &c.

For the present I shall only mention, that if the dephlogisticated air be shaken in the tube, in the manner above mentioned, not only it does not increase its bulk, but it begins to diminish from the very beginning of the operation, and it continually loses more and more of its bulk, and with its bulk of its purity.

This last mentioned property of the dephlogisticated air seems to shew, that this is a fluid much different from common air, because it has its peculiar properties by which it differs from common air not from more to less only, but entirely; as is shewn by the property this fluid has of being absorbed by water; whereas common air receives an increase of bulk and elasticity by being shaken in water.

All that I have been saying above, in order to give an idea of my method, and the words I use to express the diminutions made by the mixture of nitrous air and other kinds of respirable air, is not sufficient to obtain results constant and certain, so as to deduce any consequences from them. Even after that all the elements are
corrected,

corrected, and all the causes of error hitherto unknown or neglected by the most diligent observers, are avoided (which causes sensibly alter this kind of experiments) it is absolutely necessary to follow always a constant and equal method, not only in the act of introducing the various kinds of air into the tube, but also after the mixing of the two kinds of air. The least variation of circumstances causes very great variations in the results of the experiments, and these variations of circumstances are so minute that I never saw any of the persons that observed my experiments who could discover them, although apprized of my design. The neglect alone of this uniformity of operation may occasion an error of from 20 to 50 parts and upwards in the experiment with common air; but with dephlogisticated air the error is incomparably greater, so much that the same quality of air a moment after may decompose even a double quantity of nitrous air; so that the purest common air would appear to be noxious, and phlogisticated air; and the dephlogisticated air would appear less good, and even noxious: for, by the test of nitrous air, it might appear little different from a mixture of dephlogisticated and phlogisticated air.

I shall take another opportunity to speak of all the particulars relative to my method; but for the present I

must mention, that, after using all the cautions I am master of; the greatest error in the diminutions is not greater than one sixtieth of the common air introduced in the tube: so that, after having made five or six experiments successively, the probable error is so small that it may be safely neglected. But, if one chose to operate upon a quantity of air nine or ten times greater than that I commonly use, the error could not be 1000th part of the quantity of the air, which quantity would not be more than a few cubic inches. In the description of my method I shall also mention the means by which I obtain nitrous air of a nearly equal and constant goodness, and in what manner I can refer my experiments to some constant standard. For want of this method it is that we are not certain of the observations made about the salubrity of common air in different places, and that no tables of its changes have been made.

I have not the least hesitation in asserting, that the experiments made to ascertain the salubrity of the atmospheric air in various places, in different countries and situations, mentioned by several authors, are not to be depended upon; because the method they used was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the results very different from one another.

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When all the errors are corrected it will be found, that the difference between the air of one country and that of another, at different times, is much less than what is commonly believed, and that the great differences found by various observers are owing to the fallacious effects of uncertain methods. This I advance from experience; for when I was in the same error, I found very great differences between the results of the experiments of this nature which ought to have been similar; which diversities I attributed to myself rather than to the method I then used. At Paris I examined the air of different places at the same time, and especially of those situations where it was most probable to meet with infected air, because those places abounded with putrid substances and impure exhalations; but the differences I observed were very small, and much less than what could have been suspected, for they hardly arrived to one fiftieth of the air in the tube. Having taken the air of the hill called Mont Valerien at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the same time and treated alike; I found the former to be hardly one thirtieth better than the latter. In London I have observed almost the same. The air of Islington and that of London suffered an equal diminution by the mixture of nitrous air; yet the

air of Iflington is esteemed to be much better. I have examined the air of London, taken at different heights; (for instance, in the street, at the second floor, and at the top of the adjoining houses) and have found it to be of the same quality. Having taken the air at the iron gallery of St. Paul's cupola at the height of 313 feet above the ground, and likewise the air of the stone gallery which is 202 feet below the other; and having compared these two quantities of air with that of the street adjoining; I found, that there was scarce any sensible difference between them, although taken at such different heights.

In this experiment a circumstance is to be considered, which must have contributed to render the above mentioned differences more sensible; this is, the agitation of the air of the cupola, for there was felt a pretty brisk wind upon it, which I observed to be stronger and stronger the higher I ascended; whereas in the street, and indeed in all the streets I passed through, there was no sensible wind to be felt. This experiment was made at four in the afternoon, the weather being clear. The quicksilver in the barometer at that time was 28,6 inches high, and FAHRENHEIT's thermometer stood at 54°. After having related all these circumstances, it will be necessary to give the mean result of all the various experiments

periments made upon each of those quantities of air treated after my method with the nitrous air. The air of the street gave $11-13$, $11+6$; the air of the stone gallery, which was 202 feet high, gave $11-14$, $11+5$; and the air of the iron gallery, which was 313 feet high, gave $11-14$, $11+5$. The results of the two last experiments are exactly the same; and that of the first is hardly at all different from them. Mr. CAVALLLO, who has shewn the literary world his ability in examining nature, assisted me in those experiments, so that a mistake can hardly be suspected. From this we clearly see, how little the experiments hitherto published, about the differences of common air, are to be depended upon. In general I find, that the air changes from one time to another; so that the differences between them are far greater than those of the airs of different countries, or different heights; for instance, I have found that the air of London, in the months of September, October, and November, 1778, when treated with the nitrous air, gave $11-6$, $11+15$, which is a mean result of many experiments which differed very little from each other. The 26th day of November last, I found the air for the first time much better, for it gave $11-12$, $11+12$; but the 14th of February last, the air gave $11-18$,

II-18, II+7; from whence it appears, that the air of this 14th of February was better than it had been for six months before. There can be no doubt of the accuracy of the experiments, because I compared the air taken at different times with that which I had first used in the month of September, and which I had preserved in dry glass bottles accurately stopped. Now if the formulæ expressed above are compared together, it will be found, that the difference between the first terms is of twelve parts, and that between the latter of seven; that is, of one tenth and one twenty-fourth of the whole quantity of air: which are much greater differences than those mentioned above. Notwithstanding this, I could not perceive any particular change of health, or facility of breathing, arising from those changes of the salubrity of the atmospherical air; and I am informed, that no particular diseases appeared which could indicate any remarkable change of air.

Nature is not so partial as we commonly believe. She has not only given us an air almost equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the existence of certain kinds of noxious

air in some particular places; but only say, that in general the air is good every where, and that the small differences are not to be feared so much as some people would make us believe. Nor do I mean to speak here of those vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinsic property. This state of the air cannot be known by the test of nitrous air, and those vapours are to be considered in the same manner as we should consider so many particles of arsenic swimming in the atmosphere. In this case it is the arsenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place, therefore, I do not mean to speak of those changes which do not immediately alter the nature of the air itself. The other states of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air (the uses of which last, I shall shew on another occasion). The same thing may be said of those vapours or particles which may be good for respiration, and do not change the nature of the air. Some vegetables, for instance, can diffuse through the air such exhalations as may be of real use to the animal oeconomy when they are

are breathed for a long time, or imbibed by the pores of the skin. I remember to have often put various flowers, as roses, pinks, &c. in vessels full of common air confined by quicksilver, where I left them for several hours; after which time I found, that the air was not at all altered, but that various animals seemed to breathe it very well, notwithstanding that the flowers filled the greatest part of the vessels. On the contrary, I have found, that the vapours arising from lime slacked in water, either do not alter the air at all, or very little; though when breathed with the air they occasion the death of animals.

I would not have any body suppose, that I think it of little importance to know the goodness of the atmospherical air, and the changes it undergoes. On the contrary, I believe it to be a very useful inquiry for mankind, because we do not yet know how far one kind of air more than another may contribute to a perfect state of health; nor at what time small differences may become very considerable, when one continues to breathe the same kind of air for whole years, especially in some kind of diseases. An exact method of examining the goodness of common air may even be useful to posterity, in order to ascertain whether our atmosphere degenerates in a length of time. This curious inquiry, together
with

with the method, &c. are the production of this eighteenth century; and our descendants must have some gratitude for the philosophers who found out, as well as for those who improved it. If our ancestors had known and transmitted it to us, we should, perhaps, at present be able to judge of one of the greatest changes of our globe, of a change which very nearly interests human life.

XXIX. *Account of some Experiments in Electricity. In a Letter from Mr. William Swift to the Rev. Dr. King, F. R. S.*

Read April 29, 1779.

S I R,

Greenwich,
April 18, 1779.

BEING encouraged by the attention the Royal Society did me last year to give to the account of some electrical experiments I had made, which you were so good as to present to them, I take the liberty to beg their indulgence again, and hope you will be pleased to lay before them some new improvements I have made in my electrical apparatus. I shall esteem myself very happy if what I have done is worthy the farther attention of that most learned and respectable body.

One particular addition I have made to the apparatus consists in what I call an anti-conductor: it is exactly like the prime conductor, but it is fixed to the cushion of the machine, and consequently, when the cylinder is put in motion, the anti-conductor is charged negatively, that is, the electric matter is diminished therein in the same proportion

portion as it is increased in the prime conductor in the same time.

Another thing peculiar to this machine is, that the whole is insulated, so that being able to collect the electric matter without any connection with the earth, and having at the same time bodies or conductors positively and negatively electrified, I apprehend I am enabled by this apparatus to exhibit many experiments more analogous to the natural effects of lightning from the clouds, than it is possible to do with only one conductor positively electrified; because in nature clouds are constantly flying in the air which are differently electrified, and, discharging themselves in each other, produce the lightning often seen in the atmosphere.

I have annexed a flight sketch of the apparatus: *a* is the glass cylinder; *b* the prime conductor, on two glass stems; *c* the anti-conductor, on two glass stems, connected with the cushion of the machine; *dd* is a pole or round staff, well covered with metal, with a ball at each end, which hangs over the two conductors *b* and *c*; this staff is fastened to the cross pole *f*, which is suspended by a silk line in such a manner that the staff *dd* is equally balanced.

It may be proper to mention a few common experiments and observations, to shew, that the two conductors

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are charged and differently charged, that is, the one positively or *plus*, the other negatively or *minus*, as soon as the cylinder is put in motion by turning the wheel; though, perhaps, this will not be doubted by any persons conversant in electrical experiments, unless they have been fully persuaded, that the electric matter cannot be excited without connecting the machine with the earth, which is not the case in this apparatus, for the glass cylinder and both conductors are insulated.

1. When the cylinder moves, and a body approaches the prime conductor, such body will draw a spark from that conductor at the same distance, and consequently of the same length as will be drawn from the approaching body by the anti-conductor. And a pith ball is equally attracted by both, which sufficiently shews, that both conductors are charged or electrified.

2. The following common experiments will shew, that they are differently electrified.

I take a wire with a small piece of cocoa wood, about one inch and a half long, pointed, fastened to one end of the wire, and connecting the other end to the anti-conductor; as soon as a conducting body approaches it there is a bright spark resembling a star, which appears to settle upon the end or point of the wood; but when the wire is connected with the prime conductor, there issues

3. from

from the end of the wood a pencil of rays diverging to the point towards the approaching body, which, I apprehend, demonstrates the conductors to be differently electrified.

3. It may also be seen by another experiment. I take two jars, coated as in the Leyden experiment, and charge one by the prime conductor, the other by the anti-conductor; the first will be positively, and the second negatively electrified; which is proved by applying a discharging rod to the balls connected with the inside of the jars, when both immediately discharge themselves, which they would not do, if both jars were charged from the same conductor.

These experiments I only mention, to shew, that the two conductors are both electrified, and with this difference, that the one has more, the other less, electrical matter than in its natural state.

4. When the pole *dd* is let down by slackening the silk line on which it hangs within the sphere of action of the two electrified conductors, and being equally balanced remains *in equilibrio* over both, as soon as the cylinder begins to turn, the pole vibrates regularly towards each conductor, and as it approaches the one or the other gives a flash or spark.

lib

In this state if a ball be presented to either conductor, it makes no alteration; the pole continues to vibrate as before, even though a flash comes on the ball: but when a point is presented to either of the conductors *b* or *c*, the vibrations of the pole begin to abate, and no flash comes to the point. And as soon as any connection is made with the earth, the point being presented to one conductor, the pole attaches itself to the other with continual flashes.

5. I take two glass globes coated *ee*, and suspend them towards the ends of the pole *dd*. On turning the cylinder the pole will vibrate, and the globes will, in a short time, become charged; each globe will have its outward coat charged in the same way as the conductor over which it hangs, as will appear by discharging them into each other with a common discharging rod. This experiment shews the possibility of compressing the electric matter, though the globes are perfectly insulated.

6. While the machine remains in this state, let the prime conductor *b* be connected with other clouds, fig. 2. which are made to move over houses having conductors terminated with points; the globe *e* over the prime conductor will be thrown upwards or repelled, and the other globe only will receive a charge from the anti-conductor *c*: or if, on the contrary, the anti-conductor *c* be
I connected

connected with the flying clouds, the opposite globe only will receive its charge from the prime conductor. But if balls instead of points be made to terminate the conductors of the houses, the globes *ee* will vibrate till both be equally charged.

7. Or if we take off the globes and place a large jar or battery to each conductor, and charge them by turning the cylinder till the electrometer rises to ninety degrees, the strokes of the vibrations of the pole *dd* become very strong; then, by means of an insulated rod, I place a point on the upper part of either conductor where the knob of the pole strikes, and no explosion will come on that point. But if, instead of a point, a ball be so interposed, there will be an explosion; and the larger the ball is, the greater the explosion will be. In the latter part of this experiment, when the ball is interposed, if any person applies his finger to any wire on the board *www*, or to the solid pieces of marble standing on the board, he will be sensibly affected with a shock every time the explosion happens; but when the knob is in contact with the point in the former part of the experiment, as there is no explosion, neither will his finger be affected on the marble or any part of the wire.

8. Another experiment, which seems to agree in effect with all the foregoing, is as follows.

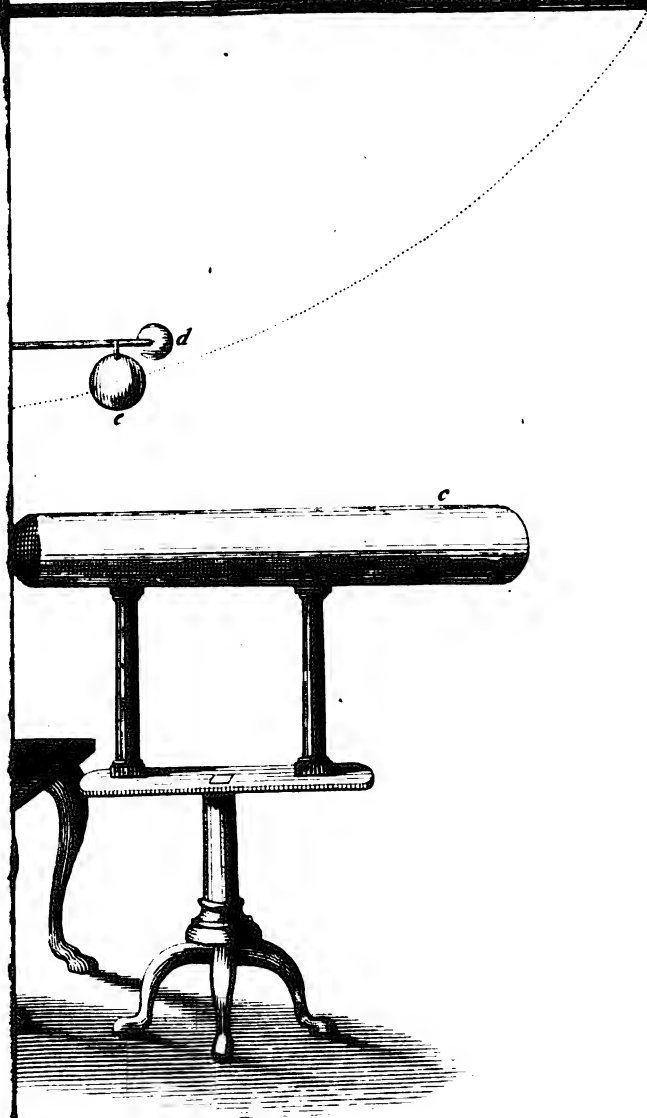
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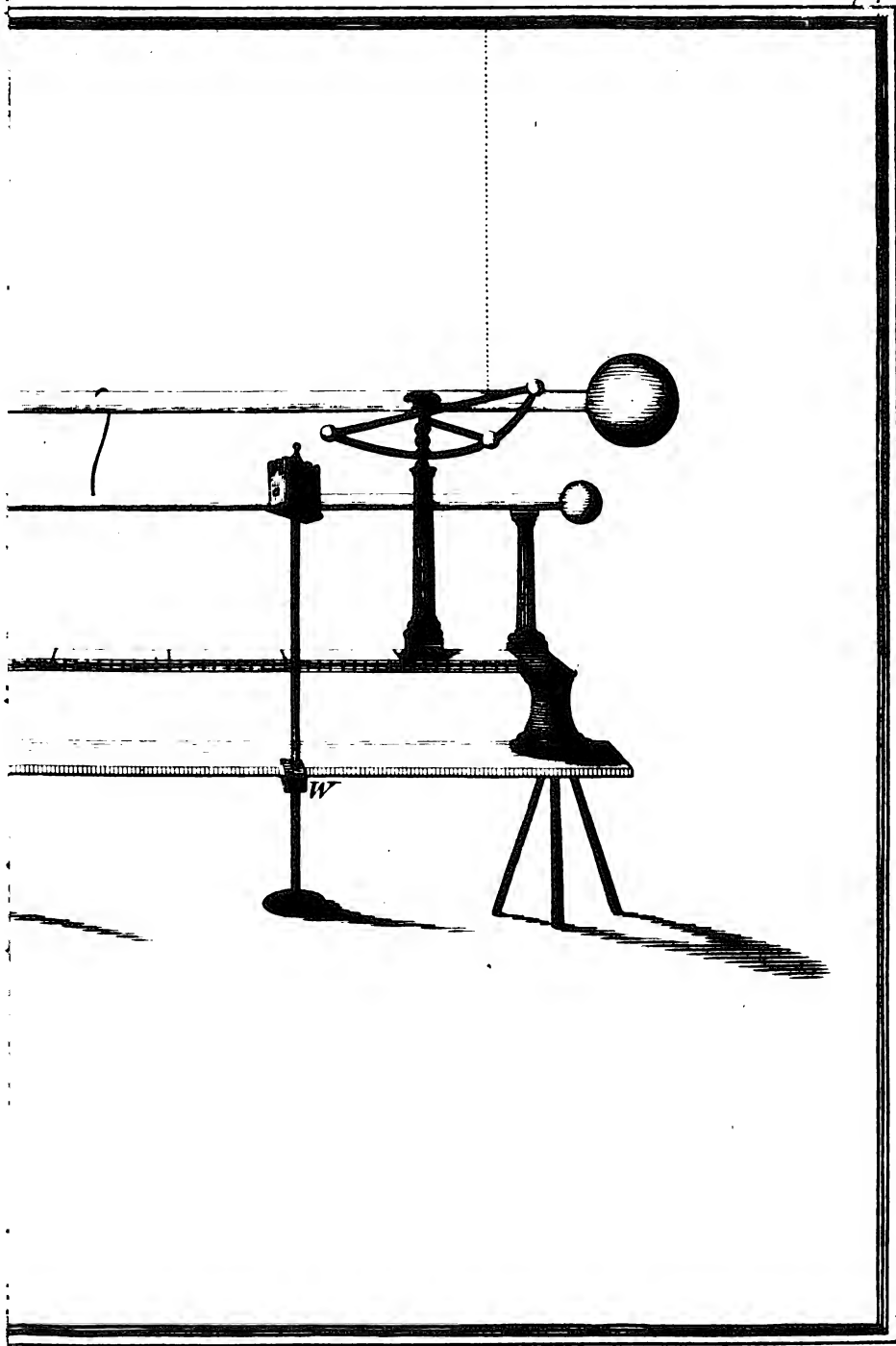
From either the prime conductor, or from the anti-conductor, I suspend a metal ball which can swing like the pendulum of a clock; I then place on a level with it, at the distance of four or five inches from it, another ball fixed and connected with the earth; as soon as the cylinder begins to turn, the swinging ball begins to be attracted, and strikes with considerable force against the fixed one, emitting a spark or flash at the same time.

In such position if a point be put the vibrations of the swinging ball will immediately begin to lessen, and it soon becomes intirely at rest, no flash or spark happening from the instant the point is put there.

9. On one of the knobs or balls at the end of the pole *dd*, I put a point turned downwards towards the conductor, and as soon as the cylinder moves it is thrown upwards or repelled, and the opposite knob or ball adheres to the conductor under it; but when points are put on both knobs and turned downwards or towards the conductors, the pole *dd* will remain unmoved, notwithstanding all the possible friction which can be given by turning the cylinder.

10. To render these experiments more analogous to the natural phenomena of lightning and rain descending from the clouds, I place a vessel of water insulated, and as the clouds (fig. 2.) being charged pass along the frame,





W. & A. S. 1766.

frame, they revolve over another cloud, which is introduced in the place of the middle house: all the houses being taken away through this cloud, the water in the insulated vessel is made to pass and to descend in a stream. When a ball is interposed between this cloud and the revolving ones, there will be frequent flashes upon it; but when, instead of a ball, a point is put there, the electric matter passes off gradually and silently without any flash.

Thus, SIR, I humbly apprehend, the whole current of these experiments tends to shew the preference of points to balls, in order to diminish and draw off the electric matter when excited, or to prevent it from accumulating; and consequently the propriety or even necessity of terminating all conductors with points, to make them useful to prevent damage to buildings from lightning. Nay the very construction of all electrical machines, in which it is necessary to round all the parts, and to avoid making edges and points which would hinder the matter from being excited, will, I imagine, on reflection, be another corroborating proof of the result of the experiment themselves.

I am, &c.



XXX. *Sitodium incisum et macrocarpon, ususque fructuum qui exinde nascuntur, descripta a Carolo Petro Thunberg, M. D.; communicated by Joseph Banks, Esq. P. R. S.*

Read May 13, 1779.

AUCTORES, qui descriptionem dederunt hujus arboris vel fructus ejus, quantum scio, pauci sunt.

RHEEDE, HENR. in Horto Malabarico, Amst. annis 1678 et seq. fol. *macrocarpon* describit.

RUMPHIUS, GEORG. EVERH. M. D. et medicus Amboinensis annos ab hinc octoginta, utriusque speciei historiam dedit, quæ invenitur in Herbario Amboinensi, a Prof. LAUR. BURMANNO, annis 1741. seq. publici juris facto.

ZANONIUS, JACOB. in Historia stirpium rariorum, Bonon. 1742, fol. *macrocarpon* memorat.

HAWKESWORTH, JOHN, Voyages round the World, Lond. 1773, vol. III. 4°, *incisi* mentionem facit.

ELLIS, JOHN, fructum hujus descripsit anno 1775, in a Description of the Mangostan and the Bread-fruit, Lond. 4°.

FORSTER,

FORSTER, JOH. REINH. et GEORG. in Characteribus generum Plantarum, p. 101 et 102. tab. 51. Lond. 4°. 1776, descriptionem genericam et figuram dederunt.

Ego, anno 1775, Bataviæ Indorum commoratus, arbores hæc characteribus botanicis descripsi et ad genus secundum sexuale systema reduxi, quæ descriptio sub nomine *Radermachia incisæ* et integræ in actis Stockholmiensibus, anno 1776, p. 250. invenienda est. Eodem anno, 1775 scilicet, semina *S. macrocarpi* misi ad hortum medicum Amstelodamensem. Postea redux e Japonia, anno 1777, plantas minutas vivas utriusque speciei plantatas ad eundem hortum e Batavia transmissi. Deinde in Europam anno 1778 ipse revertens insignem numerum mecum duxi e Ceilona, tam plantarum utriusque speciei vivarum, quam alterius speciei seminum, quæ pluribus modis conservare studui, scilicet:

1°, In chartulis, quæ theca includebantur, et aliquando aeri in umbra exponebantur.

2°, In lagenis vitreis bene obturatis.

3°, In cera.

4°, In arena ficca. Ultimæ hæ duæ methodi optimæ fuerunt. Præterea

5°, Sub ipso itinere singulis mensibus semina terræ mandavi, ut sensim excrefcere possent sub vario coelo et vario anni tempore.

SYNONYMA varia, quibus insignitur hoc genus a variis nationibus et variis in locis, sunt sequentia :

Cingallis, Herreliga.

Mallabaris, Plápallam, Tsjakamaram, Pilau.

Malais, Succong, Tjampeda, Tsjampedaha, Nancæ.

Portugallis, Jaka, Jaccas, Jaqua, Jaqueira.

Hollandis, Syrfack, Schoofack, Schrootfack, Broodboom.

Anglis, Bread-fruit Tree.

Botanicis, Soccus, Saccus, Radermachia, Artocarpus, Sitodium.

Descriptio generica.

FLORES MASCULI.

Cal. nullus. *Spadix* cylindricus vel subclavatus, sensim incrassatus, floribus undique tectus.

Cor. *Petala* duo, oblonga, concava, obtusa, villosa, filamentum includentia.

Stam. *Filamentum* unicum intra singulam corollam, filiforme, diaphanum, corollæ longitudine.

Anthera pyramidalis.

FLORES FEMINEI.

Cal. nullus. *Pericarpium* ovatum, germinibus tectum.

Cor. nulla.

Pist.

Pist. *Germinā* convexā, frequentissima, sexangulata.

Stylus filiformis, persistens.

Stigma unum vel duo, capillaria, revoluta, lineam longa.

Peric. *Bacca* ovata, muricata, carnosā, multilocularis.

Semina multiplici serie imbricata, ovata, oblique triquetra, carne molli involuta.

Character genericus. 1. Cal. o. Corol. dipetala.

2. Cal. o. Corol. o. Styl. 1.

Bacca multilocularis.

Habitatio insularis: ut in Ambona, Banda, et aliis Moluccis, Sumatra, Java, Ceilona, Maldivicis aliisque.

Descriptio specifica.

1. *S. incisum*, foliis incisif; ramulis floriferis.

Soccus lanosus. *Soccus* capas. RUMPH. Herbar. Amboin.

P. I. pag. 110, tab. 32.

Soccus granosus. *Soccun* bidji. RUMPH. Herb. Amb.

P. I. pag. 112, tab. 33.

Soccus filvestris. *Soccun* utan. RUMPH. Herb. Amb.

P. I. pag. 114, tab. 34.

Bread-fruit. HAWKESWORTH'S Voyages, vol. II. p. 80.

tab. N° 11.

Radermachia

Radermachia incisa. Ego Act. Stockholmenf. anno
1776, pag. 253.

Malais. Tjampeda, Nanca bibufang, Succong, Succon
radja, Succon timbul.

Hollandis, Bataviae. Schoorfack, Syrfack, Broodboom.

Ceilonæ. Syrfack, Maldivifche Syrfack.

Crescit species hæc sponte in insulis Amboen. Banda, et
aliis Moluccis, in Java in et circum urbem Bataviam
fatis vulgaris, in insulis Maldivicis alibique; culta in
Mallabaria ut Surattis, ad promontorium Comorin; in
Coromandelâ ut Sutucorin, Tranquebariæ, Nagapat-
nam; inque Ceilona circum Colombo, Gale, Maru-
ram, Jafnam, Trinquilimale, in insulis Marianis, &c.
Floret fructusque maturos profert, bis in annum, primis
octo mensibus.

Caulis arboreus, erectus, crassitie hominis, altitudine quin-
queorgyalî. *Rami* fastigiati oppositi, subverticillato-
quaterni, patentes. *Ramuli* subverticillato-quaterni,
floriferi.

Folia alterna, petiolata, oblonga, supra medium pro-
funde incisa, 9-lobata, integerrima, villosa-scabrida,
patentia; supra viridia nervis pallidis; subtus pallida;
bipedalia, pedem lata; juniora plicata, minora, viscida.
Petiolus crassus, subtriquetus, villosus, pellicaris. *Sti-
pula* juniora folia involventes, duæ, sessiles, lanceolatae,
I acuminatae,

acuminatæ, concavæ, integerrimæ, intus glabræ, ~~extus~~
hirsutæ, deciduæ, palmares.

Floræ in ultimis ramulis, masculi et feminei distincti in
eodem ramulo, pedunculati.

Pedunculus subteres, villosus, erectus, bipollicaris, cras-
sitie digiti.

Spadix spithamæus, cernuus, deciduus.

Pericarpium magnitudine capitis infantis, sterile vel fe-
minibus sterilibus farctum.

2. *S. macrocarpon*, foli. indivisis trifidisve; caudice ra-
misque floriferis.

Tsjakamaram. REEDE, Hort. Malabar. P. III. pag. 17.
tab. 26, 27, 28.

Saccus arboreus major. Nanca. RUMPH. Herb. Amboin.
P. I. p. 104. tab. 30.

Saccus arboreus minor. Tsjampadaha. RUMPH. Herb.
Amboin. P. I. pag. 107. tab. 31.

Jaca. ZANON. Hist. Stirp. rarior. pag. 127. tab. 90, 91.

Radermaobia integra. Ego Act. Stockholm. anno 1776,
pag. 254.

Malais. Tsjampedaha, Nanca, Nanca bubun.

Portugallis. Jacca five Jaccas.

Hollandis. Syrfack, Scoorfack, Schrootfack, et Broodboom
proprie.

Erescit

Crescit sponte in insulis Moluccis, Ambon, Banda, aliis; in Java parcius, Sumatra, Ceilona vulgaris, Coromandel, Tranquebaria, inque promontorio Comorin.

Floret fructusque bis in annum, primis octo mensibus, sensim profert.

Caulis arboreus, erectus, crassitie humana, quinqueorgyalis et ultra. *Rami* alterni, patentes, ramulosi. *Ramuli* alterni, iterum ramulosi, villis longis rigidis hirsuti.

Folia alterna, petiolata, ovato-oblonga, obtusa cum acumine obtuso, obsolete ferrata, indivisa, nervosa; supra læte viridia, lævissima; subtus pallidiora, pilis rigidis hirsuta; patentia, spithamæa. *Juniora* evidenter dentata sunt, qui dentes in adultis evanescunt. *Petiolus* subtriqueter, glaber, pollicaris. *Stipula* ut in priori. Raro folium unum alterumve incisum.

Flores masculi et feminei in caule vel ramo eodem distincti. *Pedunculus* vel simplex vel ramosus, pendulus, pollicem crassus, pedalis. *Pedicelli* tres, quinque vel plures, digitum crassi longique.

Spadix digitalis, erecto-patens.

Pericarpium magnitudine insigni, pondere 30 librarum et ultra, fertile, adeo ut omnium baccarum facile maxima hæc sit. *Semina* magnitudine tripla et quadrupla Amygdalarum, sæpe 200 usque 300, ovato-oblonga, altera

altera extremitate acuta, altera obtusa, parum complanatis lateribus.

Observationes in genere circa utramque speciem.

Arbor tota, ramī fructusque abundant succo lacteo albo, fibroso et tenacissimo, qui sine oleo abluī non potest.

Crusta extima fructuum coriacea est, tuberculis undique hispida et muricata; hac ablata semina in conspectum veniunt, quæ carne et variis tunicis circumvolvuntur. Primum inter semina deteguntur lamellæ albæ, carnosæ, tenues, lineares, semina distinguentes. Deinde singulum semen circumvolvitur tunica carnosæ, striata, immatura alba, matura flava, sapida, molli. Postea observantur duæ tunicæ tenues et feminibus magis propriæ, quarum exterior alba et siccatione secedens, interior brunnea; hæ duæ conservari debent in feminibus plantandis. Media columna in fructibus parvis valde crassa est, quo vero magis fructus accrescunt et maturantur, eo hæc tenuior evadit.

Utriusque speciei fructus, dum maturus est, odoris paululum ingrati et fere nauseosi est, licet interiora delicata sint.

Si pluviae cadant tempore florescentiae, pericarpia parva putrefacta decidunt et parca tunc erit messis; si vero aetatem quindecim dierum dudum attigerint, pluviae vix amplius nocent.

S. incisum longe vulgarius est insula Java, *macrocarpon* vero in Ceilona, ubi illi praefertur tam ob gratiorem saporem, quam ob insignem suam magnitudinem.

S. macrocarpon propagatur feminibus, quae facile excrescunt; *incisum* vero, quod sterile est, radicibus sub ipsa superficie terrae plantatis. *S. incisum* optime colitur tam in humo arena mixta quam in terra argillacea; *macrocarpon* vero amat argillam humo mixtam.

Flores fructusque primos *S. incisum* profert tertio quartove anno, *macrocarpon* vero anno quinto vel sexto: utrumque perennare dicitur annos 100 usque 150, semper fructiferum. *S. incisum* fructus suos profert in ipsis extremitatibus ramulorum, *macrocarpon* vero in ipso trunco et majoribus ramis.

Observationes circa S. incisum.

Species haec longe communissima est in orientalibus regionibus Indiae Orientalis. In Ceylonam, Mallabariam, et Coromandeliam allata fuit et radicibus propagata. In insulam Ceylonam primum ab incolis Maldivicarum insularum, qui quotannis naviculis adveniunt, annis 1727 vel

vel 1728 allatæ fuerunt radices et plantatæ intra Castellam juxta templum in horto domus proximæ. Duas has arbores proceras ipse vidi, e quibus in Ceilona plurimæ aliæ, magna copia propagatæ sunt. Ob hanc rationem hodie adhuc ab Hollandis arbor Maldivica (*Maldivische Syrfack*) appellatur.

Radices plantatæ primum post spatium duarum, trium vel plurium mensium mihi inceperunt prima germina emittere, licet sub sirio ardente et intra tropicos plerumque navigans qualibet fere vespere aqua non pauca irrogaverim.

Semina hujus magnitudine dupla pisi circumdantur carne tenuissima et delicata, sed sterilia sunt nec plantata excrefcere volunt.

Observationes circa S. macrocarpon.

Radices amant superficiem terræ sique alte sub terra imposita sepeliuntur, arbor emori dicitur, præcipue ab humo pingui.

Dum fructus maturare incipiunt, comeduntur a Sciuro palmarum atque Corvis, si non ab horum insidiis præservantur. Sciurus palmarum primus plerumque est qui vulnus infligit, et apertura ab illo facta adveniunt Corvi, illum fugantes quodque supereft fructus comedentes.

Fructus varios trunci et ramorum maturos ponderavi, qui plerumque 30 et 35 librarum pondere fuerunt.

Narratum mihi fuit a variis fide dignis, quod ipsæ radices infra superficiem terræ interdum fructus proferant, qui sub maturatione terram divellunt et adeo ponderosi sæpe evadunt, ut a duobus fervis portari debeant singuli, hique fructus maxime delicati æstimantur.

Accidit interdum, ut fortioribus ventibus dejiciantur fructus ponderosi, dum valde grandes e tenui et non satis forti pedunculo dependent. Dum valde grandes evadunt fructus et aliquot eidem insident pedunculo ut tres, quinque vel plures, tunc plerumque corbes ex foliis cocos confectæ subponuntur, ne ab hostibus suis supra memoratis lædantur, nec pericarpia adhuc immatura, præ pondere rupto pedunculo, decidant et ne noceant invidi oculi, unde fructus decidunt, ut superstitiose credunt.

Duobus vel saltem tribus mensibus fructus suos maturat hæc species, dumque primi fructus maturi sunt, iterum florere incipit.

Ceilonenses triplici nomine insigniunt hos fructus, prout illis utuntur varia ætate et prout magis vel minus maturi sunt. *Pollos* audiunt, dum adhuc parvi magnitudine ovi Struthionis, et immaturi sunt, ætate dimidiæ vel integræ mensis; hoc quoque nomen tribuitur fructibus *S. incisæ* in genere. *Herreh*, dum semimaturi sunt, magnitudinæ cocos unius cum dimidia. Caro feminum.

tunc adhuc alba, lactescens est, nec sine præparatione edulis. *Warreka*, dum plenam fructus maturitatem attigerunt; caro seminum mollis, dulcis, flava et sine prægressa præparatione edulis et delicatissima est, licet dulcedo aliquantum nauseosa sit. Maturitatem minorem vel majorem concludimus ex mollitie et sono, quem digitis pulsatus fructus plus vel minus obtusum edit. Pollos dissectus in aëre rubescit et adstringens est, sed coctus iterum albescit. Distinguunt quoque nonnulli fructus in duplicem varietatem, scilicet nucleis duris et mollibus: e mollibus conficitur *Fios*.

Hæc species in Ceilona vulgatissima est et spontanea, Cingallis pauperibus, qui nihil vel parum oryzæ possident, maxime necessaria et insignis utilitatis, dum fructum cum pauxillo rasuræ Cocos vel cum rasura Cocos et oryzæ parum comedunt.

Mos obtinuit in Ceilona semina hujus cum humo et aqua mixta in discisso Cocos externo involucri serere, ut germinent, antequam in hortis plantentur.

Licet hæc species per totum fere Orientem crescat, nullibi tamen locorum vel majores vel ponderosiores fructus vel delicates proferat, quam quidem in insula Ceilona et quibusdam Mallabarizæ regionibus, ut ipse non modo observavi oculatus testis, sed et in antiquis peregrinatorum scriptis adnotatum inveni.

USUS.

usus. *Lignum* paucorum annorum, nimis lactescens adhuc, ullius vix usus est. Cum vero arbor 20 vel 30 et 40 annos attigerit, adhiberi potest lignum pro vario usu in construendis domibus, pro laminis, trabibus; in construendis naviculis, ubi injurias aquæ et aeris 30 annos et ultra perdurat; in fabricandis sellis, scamnis, cistis, cæteris.

Lac arboris infervit pro capiendis psittaccis et aliis avibus, dum instar visci inungitur arborum ramis.

Fructus autem præprimis est, qui generi humano tantæ utilitatis est, vel sterilis vel feminibus factus.

Bacca, dum omnino matura est, scilicet *S. macrocarpi*, carnem habet semina obvestientem, mollem, flavam, dulcem et delicatam, quæ sine ulla præparatione prægressa vel sola editur vel cum oryzæ pauxillo, et optime sapit. Hæc caro in duas vel tres partes discissa cum oleo recenti Cocos affata quoque comeditur.

Bacca utriusque speciei, dum adhuc immatura est, id est Pollos et Herreli, variis modis præparata nutrimento infervit. Ingreditur præparationem Currii Caldu, Currii seco, Niembelæ, Lixationis cum lardo, Pollos tjundido, fricadellarum, Pollos affati, Empade, Pei et hinc, in futuros usus, exsiccat fructus.

Pauperes Cingalli sequenti modo fructus e *S. inciso*

cifo et e *S. macrocarpo* exemptis feminibus, præparatos adhibent. Omnia cultro in tenuissimas et minutissimas partes conscindunt, cumque Cocos raspato *aa* supra lapidem contrita vel sola comedunt vel cum additis Capficis, sale et Allio. Mēs quoque Hollandis in Indiis est, Pollos utriusque speciei in tenues lamellas concisum cumque oleo fortiter affatum comedere, juxta potum Theæ. Hæ lamellæ tenues aliquando etiam lixantur cum carne et jusculo gallinarum.

Nuclei vel cum tunica sua carnosâ, vel cum toto fructu, vel foli pro nutrimento adhibentur ut in ferculis variis, fructu cocto, Pollos tjundido, confectiione seminum, Fios placentulisque. *Nuclei* mundati etiam foli eduntur tam a ditioribus quam a pauperibus, cocti vel affati. Cocti plerumque a pauperioribus cum Cocos raspato et sale comeduntur. Ditiores interdum hisce in quatuor plerumque partes concisis et passulis mixtis vel porcos juniores vel anseres affatos, loco Castanearum, implere sciunt. Usus, quos habent medicales nuclei, tanti quidem non sunt; nihilo tamen minus Javani exinde cum lymphâ Cocos emulsionem parant, quam Diarrhœis laborantibus propinant.

Om nibus itaque prioribus, quæ homini alicujus utilitatis esse possunt, pro nutrimento et alimento adhibitis, cætera tamen ut inutilia non abjiciuntur, ut cortex fructus
externus,

externus, lamellæ paleaceæ, quæ semina circumstant, tunicæ nucleorum et columna media; sed hæc in minutas partes conscissa et cocta, suis, qui avidè devorant, apponuntur.

E columna media discissa, parum cocta et in aceto macerata Atjar conficiunt. Atjar (Pickles) vocant omne, quod aceto forti conservatur et pro condimento ciborum infervit. Ex pollos quoque Atjar fit, dum antea decorticatus et coctus aceto inditur.

Descriptio fercularum, quas e fructibus præparant Hollandi in India Orientali, præsertim in insula Ceilona.

1. *Currii Caldu.* Rc. tenuium lamellarum Pollos m ij. Coque parum cum aqua et radice Curcumæ ut flavescant. Adde Piscis ficcati (Comlimas dicti, e Maldivicis insulis adducti) et contusi p ij. si velis; lactis Cocos secundi lb ij. Coque iterum tenui igne per semihoram, aliquando agitando.
2. *Currii seco.* Rc. tenuium lamellarum Pollos m ij. Coque cum aqua et radice Curcumæ parum, ut flavescant. Adde Cocos ustæ contritæ simul cum seminibus Coriandri et piperis, cortice cinnamomi, mace et aquæ falsæ parum: lardi saliti cocti et in parva frusta concisi q. vis: aquæ, in qua coctum fuit lardum, parum.

Omnia

- Omnia hæc bene miscuntur et adduntur lactis Cocos cochlearia aliquot. Coque per dimidiam horam. Postea alio vase affatur butyro herba allii concisa, priora omnia adduntur beneque miscuntur. Additur porro succus duarum limonum, miscetur optime et paululum aquæ falsæ ad gratum saporem additur. Quidam addunt unicum Capsicum et duos allii bulbos.
3. *Pollos Tjundido.* Rc. Pollos vel Herreli cum vel sine nucleis tenue confcissorum q. vis. Coque primum cum aqua et radice Curcumæ. Adde Cocos raspatæ cum Capsico, allii bulbis concisis et sale m ij. Coque iterum leni igne massam fere ficcā, sæpe agitando et interdum vase clauso. Coctione per dimidiam horam peracta, paratum est.
4. *Currii Castaneum.* Rc. Nucleorum a tunicis mundatorum et in longis angustis lamellis concisorum circiter m ij. Coque parum cum aqua et radice Curcumæ, ut flavescant. Adde, si velis, Piscis Comlimas dicti p j: lactis Cocos secundi lbss—i: Allii herbæ concisæ parum. Coque vase clauso per dimidiam horam leni igne, interdum agitando. Adde lactis Cocos primi paululum. Coque iterum, continuo agitando, per $\frac{1}{8}$ horæ partem.
5. *Niembela.* Rc. Pollos vel Pollos et Herreli valde grosse concisorum q. vis. Adde aquæ salitæ et allii foliorum

conciforum parum: lactis Cocos primi lbj. Piscis (Comlimas dicti) contusi ꝑ ij. Coque leni igne, sæpe agitando, per dimidiam horam.

6. *Lixatus cum lardo fructus*. Rc. Pollos et Herreli grosse conciforum q. v. Adde aquæ salitæ et allii herbæ concifæ parum: lardi falsi cocti et in frustra concisi atque aqua frigida abluti q. vis: aquæ, in qua lardum coctum fuit, paululum: macis et frustulorum Cinnamomi, herbæ allii concifæ et cinnamomi pulverisati aa ꝑ ij. Coque per quadrantem horæ, aliquando quaffando.

7. *Fructus coctus*. Rc. nucleorum cum carne et tunicis maturorum, inque duas vel tres partes conciforum q. v. Coque cum aqua et radice Curcumæ q. s. addendo sub coctione paululum salis. Ita coctus comeditur a pauperibus, cum rasura nuclei Cocos; ditiores addunt Capsicum et pisces ficcatos.

8. *Lamellæ Pollos affatæ*. Rc. lamellarum tenuium Pollos coctarum q. vis: lactis Cocos lb ss: farinæ m ij. Hisce mixtis involve lamellas et in vase plano affa cum olei Cocos recentis magna copia.

9. *Empade*. Rc. lamellarum Pollos ut supra affatarum q. v. Adde herbæ allii tenue concifæ, lactis Cocos (Comlimas) piscis ficcati, bulborum allii conciforum, et pulveris Cinnamomi. Coque vase plano, leni igne,

continuo cum cochleari lac Cocos super affundendo. Per quadrantem horæ ita coctis adde allii bulborum tenue concisorum et cum butyro affatorum, quantum lubet.

10. *Fricadellæ ex Pollos.* Rc. Pollos cocti et in pulvem redacti m ij: allii herbæ concisæ, Cinnamomi, Piperis, nucis moschatæ, falis, panis biscocti aa pulverifatorum q. vis: vitellum ovi unicum. Misce optime fiantque inde boli, obvolvendi albumine ovi, ut melius cohæreant. Postea assentur butyro solo vel oleo Cocos et butyro simul, usque dum rufescant. Paratis sic bolis super affunditur embemma e butyro, pulvere cinnamomi, pipere, fale et fucco limonum confectum.

11. *Confectio seminum et carnosæ ejus tunicæ.* Rc. Tunicarum carnosarum seminum in duas vel tres partes concisarum q. v. Assa cum oleo expresso recenti Cocos. Oleum deterge cum linteo vel pone affatas tunicas in cribro, ut defluat oleum. Postea obvolve syrupo sacchari condensato, sicca et lagenis bene obturatis conserva. Per tres menses ita conservari possunt et eduntur juxta potum Theæ.

Rc. Seminum a tunicis mundatorum et integrorum. Cum oleo affata involve syrupo sacchari, sicca et ut priores conserva in eundem usum. Si femina affata

R r r 2

cum

cum ipso syrupo in lagenis obturatis includuntur, per dimidium annum conservari possunt.

- I 2. *Fios*. Rc. feminum maturorum q. v. involve pulti e lacte Cocos, farina et vitello ovi confectæ. Affa cum oleo recenter expresso Cocos.
- I 3. *Placentulæ*. Rc. Vespere Syri dicti (i. e. aceti e succo Cocos fermentato): farinæ q. v. unde fiat bolus. Altero mane, nocte peracta fermentatione, adde lactis Cocos, farinæ feminum ficcatorum q. s. et ovorum vitella pro lubitu. Fiant modo consueto placentulæ.
- I 4. *Pei* five *Jambal*. Rc. Pollos cocti, feminum sinapis aa m ij, radicis Curcumæ q. v. Omnia seorsim in pul-tem redacta, optime misceantur cum paululum aceti. Adhibetur cum aliis cibis, cum piscibus et cum oryza quandoque sola, ob gratum saporem, præsertim pauperibus. Quidam his quoque addunt Capsicum, zingiber, allium et sal commune, ut priora in pul-tem redacta.
- I 5. *Siccatio fructuum*. Rc. fructus semimatturos vel integros vel discissos exemta carne. Coque parum et postea sicca optime insolatione, et ita serva vel suspensos in Camino vel alio ficco loco. Hac ratione per annum conservari possunt. Pauperes ita ficcatos cum Cocos rasura comedunt vel etiam coctos. Dum aliquando magna copia colligunt fructus, ita illos ficcatos in futuros usus conservant.

Observationes

Observationes circa fercula præparanda.

Currii, quod communissimum Indianis est ferculum, jusculum quoddam audit, cui additur radix *Curcumæ* ut flavum induat colorem, et capsicum, ut igneus plus minus evadat sapor, præter pauca alia aromata. Ejusmodi jusculum fit e jusculo carniū ut gallinarum, anserum, anatum, ovium, pisciumque; item ex emulsione nucleorum *Cocos*, cum carnibus supra dictis vel cum carnibus et variis simul fructibus concisis ut melonibus, bilimbing (*Averrhoa Bilimbi*) aliis, vel etiam cum hisce et aliis fructibus absque carne. Dum jusculum adeo paucum est, ut fere ficcum, vocatur *Currii seco*.

Omne, quod *Currii* audit, ingredi debet radix *Curcumæ*, ut color evadat flavus et ad saporem acrem plus minus igneum, *Capsicum* et alia aromata, pro cujusque lubitu, adduntur ut *Currii Caldu*, *Currii seco*, *Currii castaneo* et *Pollos tjundido*.

Radix *Curcumæ*, quæ communi nomine in Indiis appellatur *Borriborri*, et ad finem *Currii* additur, sequentem in modum præparatur. Primo aqua maceratur ut mollis fiat, dein supra lapidem planum cum pauxillo aquæ conteritur in pulvem sinapis densioris consistentiæ. Lapis, plerumque *lapis Borriborri* dictus, planus est, plus

vel minus magnus, cum pistillo lapideo, ea longitudine ac ipsa latitudo lapis, cylindrico, altera extremitate rotundato, altera transverso. Super hunc lapidem in pulvem teruntur non modo radix Curcumæ sed etiam pollos coctus, sinapis, Capsicum, allium, Cocos raspatum, oryza tosta et alia aromata, &c.

Currii seco variâ requirit, quæ antequam in pulvem redigantur, tosta esse debent ut Cocos, femina Coriandri, cortex Cinnamomi et, ut quidam amant, oryza, piper, feniculum et cetera. Hæc singula vel separatim vel aliquot simul mixta in vase terrestri super ignem, perpetuo agitando comburuntur, donec brunnea evadant. Tunc super lapidem cum adjecta aqua in pulvem fere solidam rediguntur.

Fructus, prima et secunda sua ætate, id est, Pollos et Herreli, ante præparationem decorticantur et inciduntur. In octo plerumque partes fructus primum inciduntur et singulum frustum separatim decorticatur. Deinde singula pars inciditur in frusta vel lamellas majores vel minores, crassiores vel tenuiores pro usu diverso. Incisio hæc adeo peragitur: cultri manubrium firmum tenetur inter hallucem et proximum pedis digitum, ita ut acies superiora spectet et sub medio posito ligno apex elevetur, infra quem fructus inter ambas manus retentus inciditur. Incidentes tunc federe debent in ipso solo vel pavimento substrato

substrato tapete, vel in scamno humiliſſimo, ne altius ſedentes dorſi dolorem contrahant. Hoc modo conſcinduntur non modo fructus completi, et fructus abſque nucleis; ſed etiam nuclei, vel cum carne circumvolvente et tunicis, vel ab eis mundati. Hæc eſt methodus plerorumque Indianorum et quorundam Europæorum in Indiis degentium; ſed hæc quoque incifio ſupra menſam cultro inſtitui poteſt. Concifa omnia aqua purificantur ab arena et aliis adhærentibus immunditiis.

Nuclei a tunica carnoſa ſeparati a ceteris tunicis quoque mundantur. Exteriorem albam tunicam ſingulari modo ſeparant, ſcilicet nucleum perpendiculariter ſupra lapidem collocant et lapillo extremitatem ſupremam ſeu apicem pulſant, uſque dum diſrumpitur tunica et facile digitis ſeparari poteſt. Intimam brunneam tunicam cultro radendo eximunt.

Lac ſeu emulſio nucleorum Cocos, eodem, quo aliæ emulſiones modo, præparatur. Proprio primum inſtrumento nucleus raſpatur, aqua frigida additur, raſura inter digitos premitur et emulſio decolatur. Hoc *lac primum* vocant cum fortius ſit; ſi vero iterum raſuræ aqua ſuper infunditur et expreſſio fit, obtinetur *lac* ita dictum *ſecundum*, quod dilutius eſt.

Proportionem

484 *Sitodium incisum et Macrocarpon descripta.*

Proportionem ingredientium circum circa indicavi, secundum quam ipse vidi hæc fercula præparari; sed illa tamen pro cuiusvis gustu arbitraria est, sic ut hoc vel illud secundum placitum augeri possit vel diminui.

XXXI. *A Second Paper concerning some Barometrical Measures in the Mines of the Hartz.* By Mr. John Andrew De Luc, F. R. S.

Read June 10, 1779.

THE Royal Society having been pleased to accept the communication which I had the honour to make to it two years since of certain barometrical experiments in the mines of the Hartz, I take the liberty of communicating to it some others of them, which I made the last year in one of the deepest of the mines in those mountains, named the *Deep St. John*.

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Second Mémoire sur des Mesures Barométriques dans les Mines du Hartz. Par Mons. De Luc, F. R. S.

LA Société Royale ayant bien voulu agréer la communication que j'eus l'honneur de lui faire il y a deux ans de quelques expériences du baromètre dans les mines du Hartz, je prends la liberté de lui en communiquer d'autres, que je fis l'année dernière dans une des mines les plus profondes de ces montagnes, nommée le *Profond St. Jean*.

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It was on the 5th of July that I went down into it. I had been informed that I might not perhaps be able to arrive at the lowest galleries because of the foul air which had been there all the preceding days. Some miners therefore went before to examine how far we might go down without danger. Mr. USLAR, who had been so kind as to accompany me to the mines of Ramelsberg, was also pleased to be of this party, as well as Mr. FREDERIC, one of the officers of the mines, a gentleman well skilled in subterraneous geometry. And whilst we were under ground Mr. MAYER, one of the clerks of the office of the mines, observed his barometer above ground every quarter of an hour.

It had been fine weather for some days, and that day it was particularly so; and Mr. MAYER perceived no variation

Ce fut le 5^e Juillet que j'y descendis. On me prévint que je ne pourrais peut-être pas arriver jusqu'aux galeries les plus basses à cause des mouffettes, dont il y avoit eu tous les jours précédens. Quelques mineurs prirent donc les devans pour examiner jusqu'à où nous pourrions descendre sans danger. Mons. DE USLAR, qui avoit eu la bonté de me conduire aux mines du Ramelsberg, voulut bien encore être de cette partie, de même que Mr. FREDERIC, l'un des officiers mineurs, très au fait de la géométrie souterraine. Et tandis que nous fûmes dans l'intérieur de la terre, Mr. MAYER, l'un des officiers du bureau des mines, observa son baromètre chaque quart d'heure à l'extérieur.

Il faisoit fort beau depuis quelques jours, et ce jour là en particulier; et Mr. MAYER n'aperçut aucune variation dans son baromètre de 4 h. à 9 h. du soir:

riation in his barometer from four to nine o'clock at night, which, at that height, indicated the greatest stability in the state of the air in all other respects, excepting for the ordinary variations of the diurnal heat. Had it been on the Plain, the immobility of the barometer, in that part of the day, would have indicated that it had a tendency to descend. I shall not dwell on the cause of this difference, which results from that of the heights; I have explained it in my work on the atmosphere.

It was $4\frac{3}{4}$ h. when I made, at the entrance of the mine, the first observation of the barometer and of the thermometer which accompanies it; and at the bottom of the first ladder, that of the detached thermometer. Continuing to descend we arrived at $5\frac{1}{2}$ h. towards the middle of the depth of the mine, near the beginning of a gallery

ce qui, à cette hauteur, marquoit la plus grande stabilité dans l'état de l'air, à tout autre égard que pour les variations ordinaires de la chaleur diurne. A la Plaine, l'immobilité du Baromètre dans cette partie du jour, eût marqué qu'il tendoit à descendre. Je ne m'arrêterai pas à la cause de cette différence, qui résulte de celle des hauteurs; je l'ai expliquée dans mon ouvrage sur l'atmosphère.

Il étoit $4\frac{3}{4}$ h. lorsque je fis à l'entrée de la mine la première observation du baromètre et du thermomètre qui l'accompagne; et au bas de la première échelle celle du thermomètre détaché. Continuant à descendre, nous arrivâmes à $5\frac{1}{2}$ h. vers le milieu de la profondeur de la mine, auprès du commencement d'une

a gallery named the George, where I made an observation at $5\frac{3}{4}$ h.

The miners who had gone before us, having come back from the bottom of the mine, reported that the vapours were dissipated, and that the air was every where good. So we continued to descend to the place where, for want of a sufficiently deep draining gallery, the water is obliged to be left, although by some extraordinary efforts they had formerly sunk the shaft ten or twelve fathoms deeper. This point is at the level of the eleventh gallery. I observed there at $7\frac{1}{2}$ h. The air was perfectly wholesome, only a little warmer than at the top of the shaft.

In ascending again I repeated the observation at $8\frac{1}{2}$ h. near the George gallery; the barometer stood then one eighth of a line higher than the first time. On returning

galerie nommée George, où je fis une observation à $5\frac{3}{4}$ h.

Les mineurs qui nous avoient précédés, étant alors de retour du fond de la mine, nous rapportèrent que les mouffettes étoient dissipées, et que l'air étoit bon partout. Ainsi nous continuâmes à descendre jusqu'au point où, manque d'une galerie d'écoulement assez profonde, on est obligé de laisser l'eau; quoique, par des efforts, on ait approfondi autre fois le puits de 10 à 12 toises de plus. Ce point est à niveau de l'onzième galerie. J'y fis l'observation à $7\frac{1}{2}$ h. L'air y étoit parfaitement sain; seulement il étoit un peu plus chaud que dans le haut du puits.

En remontant, je réitérai l'observation à $8\frac{1}{2}$ h. auprès de la galerie George; le baromètre s'y tint plus haut d' $\frac{1}{8}$ de ligne que la première fois. De retour à 9 heure

ing to the top of the mine at nine o'clock I observed there again, and found the barometer one sixteenth of a line higher than when I had entered.

Combining these observations, which are set down at the end of this paper, I found that the George gallery was 127.15 lachters, or Hartz fathoms, below the entrance of the mine; and that the whole depth was 215.86 lachters, which makes 801 English feet for the first depth, and 1359 for the latter.

Mr. USLAR, having taken a memorandum of the places where I had made my observations, afterwards gave me the correspondent geometrical measures. The depth of the George gallery he found to be 127.87 lachters, that is, only four feet more than what I had found by the barometer; but I was much surprised to see that, in the whole

9 heures au haut de la mine, j'y observai encore, et je trouvai le baromètre plus haut d' $\frac{1}{16}$ de ligne que lorsque j'étois entré.

Combinant ces observations, dont les détails sont à la fin de ce mémoire, je trouvai que la galerie George étoit abaissée de 127,15 lachters, ou toises du Hartz, au dessous de l'entrée de la mine; et que la totalité de la profondeur étoit de 215,86 lachters. Ce qui fait 801 pieds Anglois pour la première profondeur, et 1359 pour la dernière.

Mr. DE USLAR ayant pris note des lieux où j'avois fait mes observations, me donna ensuite les mesures géométriques correspondentes. La profondeur de la galerie George se trouvoit être de 127,17 lachters c'est à dire plus grande seulement de 4 pieds que celle que j'avois trouvée par le baromètre: mais je fus bien

whole depth, the geometric measure exceeded mine by 110 feet.

At first I suspected an error in my observation, and I resolved to go down into the mine again. But afterwards, from a comparison of the successive observations, in which the particular construction of the scale of my barometer would have shewn the error, I saw clearly that my observation was well made. I shall not in this place explain the nature of that examination, for fear of being too long; but it is easily deduced from the construction of that scale.

I had no longer reason to think, that the barometric measure could in itself differ so much from the geometric measure, since they agreed so well in the middle of the shaft. There remained nothing to be suspected but the
geo-

surpris de voir que sur la profondeur totale, la mesure géométrique excédoit la mienne de 110 pieds.

Au premier moment je soupçonnai une erreur dans mon observation, et je me résolus de retourner dans la mine. Mais ensuite, par une comparaison des observations successives, dans laquelle la construction particulière de l'échelle de mon baromètre eût fait appercevoir cette erreur, je vis sans aucun doute que mon observation étoit bien notée. Je n'expliquerai pas ici la nature de cet examen, de peur d'être trop long; mais on le conclura aisément de la construction de cette échelle.

Je n'avois point de raison non plus de croire que la mesure barométrique pût en elle même s'écarter autant de la mesure géométrique; puis qu'elles s'accordoient si bien dans le milieu du puits. Il ne restoit donc à suspecter que la note
qui

geometric measure which had been given me, and I requested Baron DE REDEN to cause it to be examined. The person whom he charged with that care was his nephew, who is very skilful in every thing concerning the mines. He examined with Mr. RAUSCH, the chief geometer, the measure which had been given me; and they found, that in writing the depths of the different parts of the mine, the sum of which ought to give the whole depth, one of these parts had been twice put down, so that the true depth was only $215\frac{1}{2}$ lachters: it became then two feet less than that which resulted from my observations.

Thus the barometric measurement gave four feet less than the geometrical measure for the former of the depths, and two feet more for the latter.

I was

qui m'avoit été donnée de la mesure géométrique; et je priai M. le Baron DE REDEN de la faire examiner. La personne qu'il chargea de ce soin, fut son neveu, qui est très éclairé dans tout ce qui concerne les mines. Il vérifia donc cette note avec Mr. RAUSCH le géomètre en chef; et ils trouvèrent, qu'en prenant en détail les profondeurs des différentes parties de la mine, dont la somme devoit donner la profondeur totale, on avoit posé deux fois une de ces parties; de sorte que la vraie profondeur n'étoit que $215\frac{1}{2}$ lachters. Elle étoit donc alors moindre de 2 pieds, que celle qui resuoltoit de mes observations,

Ainsi la mesure barométrique avoit donné 4 pieds de moins que la mesure géométrique pour la première des profondeurs, et 2 pieds de plus pour la dernière.

Je

I was at first as much surprized as satisfied with this exactness, which I did not expect notwithstanding my former experience; and I should not have been surprized to find two or three fathoms difference either way. But, reflecting on it afterwards, I perceived that there ought to be more certainty in the barometric measure for the depth of mines, than for the height of mountains; and that for the two following reasons.

First, that in making these observations along the shafts of mines we go but little from the same vertical column, and by that we know with certainty the weight which compresses the part which we measure: whereas in mountains, even the steepest, the observations are made in columns of air which are far enough from one another to be liable to sensible differences on that account;
and

Jé fus d'abord aussi surpris que content de cette exactitude, à laquelle je ne m'attendois, point malgré mes précédentes expériences; et je n'aurois pas été étonné de trouver 2 au 3 toises d'écart dans l'un ou l'autre sens. Cependant, y réfléchissant ensuite, j'ai compris qu'il doit y avoir plus de sûreté dans la mesure barométrique pour la profondeur des mines, que pour la hauteur des montagnes; et cela par les deux considérations suivantes.

La première, qu'en faisant ces observations le long des puits des mines, on s'écarte peu d'une même colonne verticale; et l'on connoit ainsi sûrement le poids qui comprime la partie qu'on mesure. Aulieu que dans les montagnes, même les plus rapides, les observations se font dans des colonnes d'air assez éloignées l'une de l'autre, pour qu'il puisse y avoir des différences sensibles à cet égard; et
que

and that if, for example, the higher barometer were moved horizontally as far as the column which rests upon the station of the lower one, it would not stand at the same point as when it was on the mountain; which, however, is supposed in the formula.

The other circumstance respects the homogeneity of the air. In the open air a thousand lateral causes may introduce into the portion of the column which we measure some layers of air different from those of the two extremities, were it only in point of heat. And in that respect the correspondent observations nearest the foot of the mountain are not always the most certain, because of the reflexions of the sun, and even of the winds which carry the vapours; and the result must be that we very often find the heights too small. I shall not stop to
prove

que par exemple, si le baromètre supérieur étoit avancé horizontalement jusqu'à la colonne qui pèse sur la station inférieure, il ne se tint pas au même point que sur la montagne; ce qui néanmoins est supposé par la formule.

L'autre circonstance regarde l'homogénéité de l'air. En plein air, mille causes latérales peuvent introduire dans la portion de colonne qu'on mesure, des couches d'air différentes de celles des deux extrémités; ne fût-ce que différemment chaudes. Et à cet égard les observations correspondentes les plus près du pied des montagnes ne sont pas toujours les plus sûres à cause des réflexions du Soleil, et de celles des vents qui charient les vapeurs: et il doit en résulter très souvent qu'on trouve les hauteurs trop petites. Je ne m'arrêterai pas à le mon-

prove it. In mines, on the contrary, the air being inclosed as in a canal, where it is always in motion, it continually intermixes and becomes more homogeneous: it contracts there a temperature which is more uniform or more gradually different, so that we can more certainly fix the mean state of it.

On these two accounts, therefore, barometric measures ought to be more regular in most mines than on the sides of mountains; because the observations are made nearly at the extremities of one and the same column, and because the whole column is more nearly similar to its extremities; consequently we are surer that the particular case is not an exception to the rules. Now it has happened, that in all the observations which I have made in the Hartz mines, my rules, drawn from a mean among my observations in
open

trer. Dans les mines au contraire, l'air étant renfermé comme dans un canal, où il est toujours en mouvement, se mêle sans cesse et se rend plus homogène: surtout il y contracte une température plus uniforme ou plus graduellement différente; en sorte qu'on peut plus sûrement en fixer le terme moyen.

Les mesures barométriques doivent donc, par ces deux considérations, être plus régulières dans la plupart des mines, que sur les pentes des montagnes; c'est à dire parce que les observations sont faites à peu près aux extrémités de la même colonne, et que la totalité de la colonne a des rapports plus réguliers avec ses extrémités. Ainsi l'on est plus sûr que le cas n'est pas une exception aux règles. Or il se trouve en même tems, que dans toutes les observations, que j'ai faites dans les mines du Hartz, mes règles, tirées du milieu entre mes observations en
plein

open air, have agreed with the geometric measures, which seems to indicate that the usual state of the air of the mines is a mean among the different states of the exterior air as found in the whole of my observations; the almost constant state of the heat in the mines may be the principal cause of it.

As to the geometric measures themselves, I might here give some very interesting proofs of their exactness by the description of the astonishing works which are undertaken in consequence of them; but I should be afraid of being too tedious. And, besides, we may depend on the miners for the exactness of those measures, they being more interested in them than we are. I shall, therefore, conclude this paper with some general remarks on barometrical measurement.

So

plein air, s'y sont accordées avec les mesures géométriques; ce qui paroît indiquer, que l'état habituel de l'air des mines, est moyen entre les divers états où il s'est trouvé dans la totalité de mes expériences; et l'état presque constant des mines quant à la chaleur, pourroit bien en être la principale cause.

Quant aux mesures géométriques elles mêmes, je pourrois donner ici des preuves très intéressantes de leur exactitude, par le tableau des ouvrages étonnans qu'on entreprend d'après elles; mais je craindrois d'être trop long. Et d'ailleurs nous pouvons nous en rapporter aux mineurs sur l'exactitude de ces mesures; ils y ont un plus grand intérêt que nous. Je conclurai donc ce mémoire par quelques remarques générales sur la mesure barométrique.

T t t 2

Tant

So long as in this measurement we shall consider as given only the differences in the weight and heat of the air at the places of observation, we shall be subject to errors; because there are many other causes of modification in the air: and all the exactness to which we can pretend will be to determine a formula which preserves a mean among the possible variations.

This is what I have proposed to do in my own formula, and it seems to answer this end. In the different trials which have been made of it, it has sometimes given the heights too great, at other times too little, without distinction of climate. Thus, for example, the experiments at Spitzbergen by Lord MULGRAVE, and at the Pike of Teneriffe by Mr. DE BORDA, one of the French academicians, gave the heights too great; those of
Col.

Tant qu'on n'aura pour données dans cette mesure, que les différences de poids et de chaleur de l'air aux lieux des observations, on sera sujet à des erreurs; parce qu'il y a bien d'autres causes qui modifient l'air: et toute l'exactitude à laquelle on pourra prétendre, sera de déterminer une formule qui garde le milieu entre les écarts possibles.

C'est là ce que je me suis proposé dans la mienne, et elle parait répondre à ce but. Dans les diverses épreuves qui en ont été faites, elle a donné quelquefois les hauteurs trop grandes, d'autres fois trop petites, sans distinction de climat. Ainsi par exemple; éprouvée au Spitzberg par mylord MULGRAVE, et au Pic de Ténériffe par M. DE BORDA l'un des Académiciens François, elle a donné les hauteurs trop grandes; et dans les observations de M. le Col. ROY et de M. le Chev.

COL. ROY and SIR GEORGE SHUCKBURGH*, made in mean latitudes, and partly in the places where I myself had observed, gave them too little.

These differences, do not seem to depend on the climate, and indeed I have frequently observed them to happen in the same places. Thus, for example, my observation on the Glaciere de Buet, cited by SIR GEORGE SHUCKBURGH, gives the height of that mountain a little less than the geometrical measure; but MR. DE SAUSSURE having repeated the barometrical observation, it agreed with that measure by the same formula; and MR. MARC PICTET, by a third observation, found the height a little too great. In these three observations, the particulars of which I omit, the corresponding point was Geneva, distant about ten or twelve leagues.

At

Chev. SCHUCKBURGH, faites dans des latitudes moyennes, et en partie dans les lieux où j'ai moi même observé, elle les a donné, trop petites.

Ces différences ne paroissent donc pas tenir aux climats; et en effet je les ai observées frequemment dans les mêmes lieux. Ainsi par exemple, mon observation sur la Glaciere de Buet, citée par M. le Chev. SCHUCKBURGH*, donne la hauteur de cette montagne un peu moindre que la mesure géométrique: mais M. DE SAUSSURE ayant répété l'observation barométrique, se trouva d'accord avec cette mesure, par la même formule; et M. MARC PICTET, dans une troisième observation, trouva la hauteur un peu trop grande. Dans ces trois observations, dont je ne rapporte pas les détails, le point correspondant étoit Genève, distant de 10 à 12 lieues.

* Phil. Trans. 1777.

A cette:

At that distance there are doubtless some causes of variations which are irremediable; since the formula supposes that the observations are made in the same column of air. It is therefore only in the cases in which that supposition approaches near the truth that we can hope to perfect the rule. But this can only be by introducing new conditions into it; that is, other modifications of the air, of which we have not as yet taken any account.

In meditating on the causes of the diversity of results in experiments, it has always appeared to me, that the differences of the effects of heat on the air, according to the different states of it, was the principal; that is, that the air not being always of the same nature, heat, that grand cause, whose effects we ought principally to determine, does not always act equally. Besides the particular experiments

A cette distance sans doute il y a des causes d'écarts qui sont irrémédiables; puisque la formule suppose que les observations sont faites dans une même colonne d'air. Ce n'est donc que pour les cas où cette supposition approche de la vérité, qu'on peut espérer de perfectionner la règle. Mais ce ne sera qu'en y introduisant de nouvelles conditions; c'est à dire d'autres modifications de l'air dont on n'a pas tenu compte jusqu'ici.

En étudiant les causes de la diversité des résultats dans les expériences, il m'a toujours paru, que la différence des effets de la chaleur sur l'air, suivant qu'il est lui même constitué, étoit la principale: c'est à dire, que l'air n'étant pas toujours de même nature, la chaleur, cette grande cause dont il faut principalement déterminer les effets, n'y agit pas toujours également. Outre les expériences particulières,

periments which prove it, we can attribute to these differences only those of the results of the researches of some philosophers concerning the dilatations of the air by heat, applied to various physical uses.

In a paper lately read at the Royal Society, on the subject of refractions, I analysed and compared different formulæ of this kind, given by philosophers on whom we can depend. The result of that examination was that, supposing the volume of air = 1000 when the English thermometer of FAHRENHEIT is at 32° , if the heat of the air be increased 22,8 degrees of this thermometer, its volume will be,

According

particulères, qui le prouvent, on ne peut attribuer qu'à ces différences, celles des résultats des recherches de divers physiciens sur les dilatations de l'air par la chaleur, appliqués à divers usages physiques.

C'est à l'occasion des réfractions, que, dans un mémoire lu il y a peu de temps à la Société Royale, j'ai analysé et comparé différentes formules de cette espèce, données par des physiciens, sur qui l'on peut compter. Il est résulté de cet examen, qu'en supposant le volume de l'air = 1000 lorsque le thermomètre Anglois de FAHRENHEIT est à 32° , si la chaleur de l'air augmente de 22,8 degrés de ce thermomètre, son volume sera:

Suivant

According to Mr. l'Abbé DE LA CAILLE,	1040
- - - Mr. Professeur MAYER,	1046
- - - Mr. BONNE, - -	1047,7
- - - Sir GEORGE SHUCKBURGH,	1050,5
- - - Dr. BRADLEY, -	1054,4

Here then are great differences in this point only, namely, the effect of heat on the density of the air; differences which must have a visible effect in all meteorological phenomena. Doubtless they proceed, in a great measure, from the different degrees of dryness of the air, which we can no longer doubt of, since the interesting experiments of Col. ROY with the manometer*. This is the same cause to which I imputed the greatest variations in my experiments in open air, and which obliged me to conclude

Suivant M. l'Abbé DE LA CAILLE,	1040
Suivant M. le Prof. MAYER,	1046
Suivant M. BONNE,	1047,7
Suivant M. le Chev. SHUCKBURGH,	1050,5
Suivant M. le Dr. BRADLEY,	1054 4

Voilà donc de bien grandes différences sur ce seul point, l'effet de la chaleur sur la densité de l'air; différences qui doivent agir dans tous les phénomènes météorologiques. Elles procèdent sans doute en plus grande partie des différents degrés de sécheresse de l'air; on ne peut plus en douter depuis les intéressantes expériences de M. le Col. ROY avec le manomètre *. C'est cette même cause que j'avois soupçonnée de produire le plus d'écarts dans mes expériences en plein air, et qui m'obligea à conclure ma correction pour les effets de la chaleur sur

* Phil. Trans. 1778.

conclude my correction for the effects of heat upon the air from a mean among my numerous observations. Now this mean, reduced to the same term of comparison as above, is 1047, which is also a mean among those different results.

These differences therefore shew us, that atmospheric air is not a substance that is homogeneous and constantly the same; and that next to the care of making as many observations as possible in the mass of air itself which it concerns us to know, the only means of determining more exactly its various influences is to multiply our meteorological instruments.

Barometrical

l'air, par un milieu entre mes nombreuses observations. Or ce milieu, réduit au même terme de comparaison ci dessus, est 1047, qui se trouve être aussi le milieu entre ces divers résultats.

Ces différences nous montrent donc; que l'air atmosphérique n'est pas une substance homogène et constamment la même; et qu'après le soin de faire autant qu'il est possible, les observations dans la masse même de l'air qu'il nous importe de connoître, le seul moyen de déterminer plus exactement ses diverses influences, est de multiplier nos instrumens de météorologie.

*Barometrical Observations in the Deep St. John,
July the 5th, 1778.*

	Time.	Bar. corrected by its therm. Inches.	Therm. in the air.
At the ent. of the mine,	4 50 night	26.6224	- 5
	2 5 night	26.6276	- 7 $\frac{1}{2}$
The mean,	-	<u>26.625</u>	<u>- 6$\frac{1}{4}$</u>
At the George Gallery,	5 50 night	27.4167	- 17
	8 15 night	27.4271	- 17
The mean,	-	<u>27.4219</u>	<u>- 17</u>
At the level of the 11 th G.	7 30 night	27.9792	- 8

Calculation.

Observations barométriques au Profond St. Jean, le 5^e Juillet, 1778.

	h	Bar. corrigé par son therm. P. L.	Therm. dans l'air.
A l'entrée de la mine,	4 50 soir	26.7 $\frac{1}{3}$ $\frac{1}{2}$	- 5
	9 5 soir	26.7 $\frac{1}{3}$ $\frac{1}{2}$	- 7 $\frac{1}{2}$
Terme moyen,		<u>26.7$\frac{9}{16}$</u>	<u>- 6$\frac{1}{4}$</u>
A la galerie George,	5 50 soir	27.5	- 17
	8 15 soir	27.5 $\frac{3}{16}$	- 17
Terme moyen,		<u>27.5$\frac{1}{16}$</u>	<u>- 17</u>
Au niveau de l'onzième gal.	7 30 soir	27.11 $\frac{1}{8}$	- 8

Calcul.

Calculation of the Depth of the George Gallery.

Barometer at the gallery,	27.4219 = 5265 (192ds of an inch)	Log. 37213984	
At the entr. of the mine,	26.6250 = 5112	37085908	
		<u>Difference</u>	128076
Heat of the air at the gallery,	- 17		
At the entrance of the mine,	- 6 $\frac{1}{4}$		
	<u>- 23$\frac{1}{4}$ = $\frac{23\frac{1}{4}}{1000}$</u>	to deduct	2977
	1000ths of a French fathom,		125099
$\frac{1}{8}$ r to add,	- - -		2051
		<u>Lachters</u>	127.150

Calculation

Calcul de la profondeur de la Galerie George.

Bar. à la Gal.	-	27.5.1 = 5265 (16 ^{mes} de lig.)	Log. 37213984	
A l'entrée de la mine,		26.7.8 = 5112	37085908	
			<u>Difference</u>	128076
Chaleur de l'air à la Gal	-	- 17		
A la entrée de la mine,	-	- 6 $\frac{1}{4}$		
		<u>- 23$\frac{1}{4}$ = $\frac{23\frac{1}{4}}{1000}$</u>	à deduire	2977
		1000 ^{me} de toise de France,	-	125099
		$\frac{1}{8}$ r à ajouter,	- -	2051
			<u>Lachters</u>	127,150

Calculation of the Depth of the 11th gallery.

Barometer at the gallery,	27.9792 = 5372	Log. 37301360
At the entrance of the mines,	26.6250 = 5112	37085908
		<hr/>
		Difference 215452
Heat of the air at the gallery,	- 8	
At the entrance of the mine,	- 6 $\frac{1}{2}$	
	<hr/>	
	- 14 $\frac{1}{4}$ = $\frac{14\frac{1}{4}}{1000}$	to deduct 3070
		<hr/>
	1000dths of a French fathom,	212382
	$\frac{1}{81}$ to add, - -	3482
		<hr/>
		Lachters 215,864
		<hr/>

Calcul de la profondeur de l'onzième galerie.

Bar. à la galerie,	- 27.11 $\frac{2}{5}$ = 5372	Log. 37301360
A l'entrée de la mine	- 26. 7 $\frac{3}{10}$ = 5112	37085908
		<hr/>
		Difference 215452
Chaleur de l'air à la gal.	- 8	
A l'entrée de la mine,	- 6 $\frac{1}{2}$	
	<hr/>	
	- 14 $\frac{1}{4}$ = $\frac{14\frac{1}{4}}{1000}$	à deduire 3070
		<hr/>
	1000 ^{me} de toise de France,	212382
	$\frac{1}{81}$ à ajouter, - -	3482
		<hr/>
		Lachters 215,864
		<hr/>



XXXII. *On the Precession of the Equinoxes produced by the Sun's Attraction.* By the Rev. Mr. Isaac Milner, M. A. and Fellow of Queen's College, Cambridge; communicated by the Rev. Dr. Shepherd, F. R. S.

Read June 24, 1779.

IF the actions of the Sun and Moon upon the different parts of the earth were equal; or if the earth itself were perfectly spherical, and of an uniform density from the center to the surface; in either case the attractions of those remote bodies would have no effect on the position of the terrestrial equator, and the equinoctial points would constantly be the same in the heavens. But it was impossible to give the earth a rotatory motion round an axis without giving at the same time a centrifugal force to its parts. This force is greatest at the equator, and is in a contrary direction to that of gravity; on either side of the equator the force is less; and, moreover, only part of its effects is opposed to that of gravity. It is usual in determining the figure of the earth to consider the whole mass as in a state of fluidity, and the different columns as sustaining one another at the center. If the earth.

earth be considered as a hard body, firmly cohering in its parts by some other force besides that of gravity, it does not seem necessary that the different columns should be supposed to sustain each other at the center, though in both cases the direction of gravity must at every point of the surface be perpendicular to the tangent of the figure. But we know, that there is a considerable quantity of water upon the surface of the earth; and, therefore, if the equatorial regions were not higher than the polar, they certainly would be overflowed by the Ocean, which is contrary to experience; and for this reason the proportion of the diameters of the earth, determined upon the false supposition of an entire fluidity, cannot differ much from the truth.

§ 2. But the precession of the equinoxes, which depends upon the unequal actions of the Sun and Moon on the protuberant parts of the earth at the equator, will not be the same in these different hypotheses; at least we can never be certain that it will be so until we have computed their effects, and the computation itself must proceed on different principles. Suppose the earth to be fluid under the form of an oblate spheroid; or, what is more simple, suppose the region of the equator to be surrounded with a ring of fluid matter, and the unequal action of the Sun will disturb the figure of the ring, and communicate

communicate a motion to its parts. Suppose we knew the precise disturbing force of the Sun upon any one particle of this ring according to its situation; in that case we could easily find the velocity which would be communicated to such a particle in any given time; but the mutual actions of the fluid particles upon each other could never be exactly estimated, much less their effects in endeavouring to turn the whole earth round its center. However, it is easy to see, that in the case of a hard ring of matter cohering close with the surface of the earth at the equator, both the law by which the particles act on each other, and on the whole mass of the earth, will be widely different from the case of fluidity, and the effects much greater in altering the position of the axis of rotation.

To explain this by an easy example (fig. 1,) let A, B, and c, represent three small bodies in the same horizontal line AE. Suppose A to descend by any accelerating force as gravity; B to descend by the same force, a less or a greater; and c not to be acted upon at all: in every one of these cases the bodies A and B will descend with their respective velocities, and the body c will preserve its situation. If A and B are small particles of fluid of any form, and c a hard one, and if the particle A be placed

in contact with B, have its center of gravity a little above the center of gravity of B, and is acted upon by the greater accelerating force; in this case we may conceive how the action of A may disturb the motion of B, and in the same way how the hard particle c may receive a small motion from the actions of A and B. Then this motion must be extremely little compared with the whole motions of A or B, and still a great deal less if c be strongly connected with a string of hard particles along the line CE, so that c cannot be moved without the whole line CE turning round the immoveable center E. Now if A, B, and c, be supposed hard particles firmly connected to the lever AE, then it is plain that the velocity of c, whatever it is, must be in proportion to that of A and B as their respective distances from E the center of motion, and this, whatever the impulsive forces are with which A and B are urged in their respective directions.

The body c being still supposed void of gravity, let the bodies A and B be urged by forces perpendicular to AE in any small equal times through the unequal spaces s and s' , and let the magnitudes of the bodies be represented by A, B, and c respectively. Then the space through which A is actually urged in that time will easily appear from mechanics to be represented by

$\frac{A \times AE \times s + B \times BE \times s \times AE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$, and the space described by c is

$\frac{A \times AE \times s + B \times BE \times s \times CE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$. See article the 13th.

§ 3. The preceding article being well understood, whatever doubts may remain concerning the motion of a ring of matter considered as detached from the earth, we may be certain that the motion of the nodes of the equator can never be the same, whether we suppose the ring at the equator to be fluid and to rest upon the surface of the earth, partaking of the diurnal motion, or whether we suppose it hard and compact, and by its cohesion communicating a proportional degree of motion to the different parts of the earth. In fact, the problem of the precession of the equinoxes, which has hitherto been considered as extremely difficult, and in its solution drawn out by authors to an immeasurable length, requires no principles but the received doctrine of motion, and the application of the lever, which have been made use of in the last article. In that article we supposed the bodies A and B to be impelled by different forces in parallel lines, and we estimated the real space, which either A or c in any small time would describe in consequence of those impulsive forces and their mutual connection by an inflexible lever. Now this is precisely what is re-

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quired

quired to be done in the case of the Sun's unequal action on the protuberant parts of the equator. The excesses or defects of that unequal action are to be considered as forces applied to those parts, which would move them according to the different circumstances through unequal spaces proportional to the forces in equal times of action, provided the particles were at liberty to move freely in the directions in which they are urged; and, lastly, the real space must be computed through which a particle moves at some known distance from the center of the earth in consequence of these various forces. This whole process will not differ from the easy example already described, except in the length of the calculation, and the proper management of the doctrine of fluxions; and it seems advisable in difficult subjects always to begin with simple instances before we proceed to those which are more complex, and to distinguish the algebraical operations from the principles upon which they are founded.

§ 4. In order to determine how much any particle of the earth is affected by the unequal action of the Sun (fig. 2.), let $CADB$ represent the earth, s the Sun at a great distance, and CD a plane perpendicular to the line sr joining the centers of the Sun and earth. If sk or sr represent the accelerating force of the Sun on a particle at the earth's center, and sl be taken to sk in the duplicate ratio

ratio of ST to SP ; SL will represent the attraction on any particle P , and by the resolution of motion Tm or PL will represent the perturbing force of the Sun on the same particle. By the construction $SL : SK :: SK^2 : SP^2$, and by division $KL : SK :: SK^2 - SP^2 : SP^2 :: SK + SP \times PK : SP^2$ and PL or Tm is nearly equal to $3PK$, and as $3PK$ is to SK or ST , so is the space described by P in any small time in the direction PK , to the space described in the same time by the center of the earth in consequence of the Sun's attraction. This last space is equal to $\frac{z^2}{2ST}$ where z represents the arc described by the earth's center during any small motion in its orbit, and the former is equal to $\frac{3PK \times z^2}{2ST}$. This is the space which would be described by P in the direction PK if the particle was at liberty to move freely. Let us at present suppose that no other particle is disturbed by the Sun's attraction except this one, and then proceed to enquire into the effects of this disturbance when P by its cohesion communicates a motion to the different parts of the earth, which is farther constrained to turn round an axis T , the common intersection of the plane cd and the terrestrial equator. From the laws by which motion is communicated, and the property of the lever, it easily appears, as in the second article, that the space through which any particle of the

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earth's

earth's equator is impelled at the greatest distance from the axis τ , is to $\frac{3PK\kappa^3}{23T^2}$, the space which would be described in the same time by any particle at liberty, the magnitude of which is represented by P , as $P \times K\tau \times$ the radius of the equator to the sum of all the particles of the earth multiplied into the squares of their respective distances from the said axis.

To compute this sum in the easiest way, and by an approximation, which is quite sufficient when the polar and equatorial diameters differ little from one another; let DPE (fig. 3.) be a sphere whose radius is unity, divided into an infinite number of thin cylindrical surfaces, whose bases are the circles NAQ ; it is obvious, that all the particles in any one of these surfaces are at the same distance $CA=x$ from the axis of motion perpendicular to the plane of the circle NAQ . Call AP, y , and A , the area of the circle DPE and the fluent of $4Ax^3\dot{x}y$, or of $-4Ax^2y'\dot{y}$, because $x\dot{x} = -y\dot{y}$ gives the sum of all the particles in the sphere multiplied into the squares of the respective distances from the axis. This fluent corrected is equal to $\frac{8A}{15}$, and must now be diminished in the ratio of 1 to $1-2p$, if we suppose the earth to be an oblate spheroid whose equatorial diameter is to the polar as 1 to $1-p$;

and, lastly, the space described by a particle at the greatest distance from the axis is equal to $\frac{45P \times PK \times KT \times z^2}{16A \times ST^2 \times 1 - 2p}$.

§ 5. In fig. 4. let $PIA\hat{p}DK$ represent the earth orthographically projected on the plane of the solstitial colure, P, \hat{p} , the poles, IK a lesser circle parallel to the equator, and $pape$ a sphere described with the polar radius PT : then, since the particles without the globe only are concerned in changing the position of the axis of rotation, let L represent such a particle situated in the circumference of the circle IK , and by the preceding article its effect will be $\frac{45L \times LM \times MT \times z^2}{16A \times ST^2 \times 1 - 2p}$, and by the same way of reasoning, when two equal particles L, l , are supposed to be disturbed by the Sun's attraction, the space described by that point of the equator, which is at the greatest distance from the axis of rotation or the common intersection of the plane CD and the equator A , will be equal to $\frac{45z \times L \times LM \times MT + l \times lm \times mT}{16ST^2 \times A \times 1 - 2p}$, and the same argument holds for every other particle without the sphere.

The sum of all the $L \times LM \times MT + \&c.$ must now be found; and for this purpose Sir ISAAC NEWTON'S construction is, perhaps, as convenient as any that has hitherto appeared. In the same figure Nn is parallel, and xy perpendicular, to CD ; take $Lx = xl$, and let m, n , represent

sent the sine and cosine of the angle CTP to the radius unity. It is easy to prove in his way that $L \times LM \times MT + l \times lm \times mT$ is equal to $2L \times m \times n \times \overline{Lx^2 - Tx^2}$, and the fluent of Lx^2 multiplied into the fluxion of the circular arc Lx is easily found in the following manner, without having recourse to tables of fluents, or the methods of continuation.

From a known analogy the fluxion of the arc Lx is to the fluxion of its versed sine as the radius ix of the same circle to Lx the right sine. Lx multiplied into the fluxion of the versed sine is the fluxion of the area of the semi-circle Ll , and calling ix, y , the fluent of Lx^2 multiplied into the fluxion of the arc Lx is evidently equal to $\frac{y^3 A}{2}$, where A still represents the area of a circle whose radius is unity: Ay is equal to the semi-circumference ix and $Ay \times Tx^2$ is equal to the fluent of Tx^2 multiplied into the same fluxion, and calling Tx, v , and substituting for ix its equal py , the sum of all the $L \times LM \times MT + \&c.$ in the annulus ii is equal to $mnpA \times \overline{y^4 - 2y^2v^2}$. This last quantity multiplied into the fluxion of v , and the fluent taken by the common method when v is equal to TP or unity nearly, comes out $\frac{4pAmn}{15}$ and twice this quantity gives the sum of all the $L \times LM \times MT$, without the whole sphere pap , and therefore the space described by a particle of the equator

in the circle of the Sun's declination while the center of the earth is carried through the space \dot{x} of its orbit is equal to $\frac{3pmn\dot{x}^2}{2sT^2 \times 1 - 2p}$, and may be supposed to be equal to $\frac{3pm\dot{x}^2}{2sT^2}$, the alteration produced by the correction in art. 4. on account of the spheroidical figure of the earth being too inconsiderable to affect the conclusion.

§ 6. We are to observe, that the space $\frac{3pmn\dot{x}^2}{2sT^2}$ described by that point of the equator, which is the intersection of the circle of the Sun's declination, is generated by the perpetual attraction of the Sun. This attraction may be reckoned constant during the very small time of the earth's describing \dot{x} in its annual motion; and, therefore, the said point of the equator, at the end of that time, will have acquired a velocity which would carry it through $\frac{3pmn\dot{x}^2}{sT^2}$ in the same time.

§ 7. Let T represent the time of the earth's revolution in its orbit, t the time of its rotation round its axis, and suppose \dot{w} to be a small arc similar to \dot{x} in a circle whose radius is unity. In figure 5. let AQ be the equator, and take Ab equal to $\frac{\dot{w}T}{t}$, and bt perpendicular to Ab equal to $3pmn\dot{w}^2$, and Ab , bt , will represent the directions and quantities of the two different motions of the point A , and consequently At will be the direction of the

new equator, and as Ab or At is to bt , so is the radius unity to the sine of the angle tAb , and if AQ or AG be taken equal to a quadrant, GQ the measure of the angle GAQ is equal to $\frac{3pm\dot{w}t}{T}$.

§ 8. Suppose s the Sun's place in the ecliptic ns , N the equinoctial point, NA the Sun's right ascension, and ro a perpendicular on AN : then ro is to GQ as the sine of AN to the radius, and rN to ro as the radius to the sine of the angle at N the inclination of the ecliptic to the equator, and, *ex æquo perturbatè*, rN to GQ as the sine of AN to the sine of N and rN the small precession of the equinoxes is equal to $3pmn \frac{\dot{w} \times t}{T} \times \frac{\sin. NA}{\sin. N}$.

§ 9. In the spherical triangle ASN , the sine of $AN = \cotang. N \times \tang. AS = \frac{m \times \cos. N}{n \times \sin. N}$, and farther the sine of $sN = \frac{m}{\sin. N}$ and rN is equal to $\frac{3pt \times \sin. u}{T} \times \dot{w} \times \cos. N$, whose fluent or $\frac{3pt}{4T} \times 2w - \sin. 2w \times \cos. N$ gives the precession of the equinoxes during the Sun's motion through the arc ns of the ecliptic: when ns is equal to a circle, then the whole fluent becomes equal to $\frac{3pt\Lambda}{T} \times \cos. N$, and as $2T$ is to $3pt \times \cos. N$, so is $60 \times 60 \times 360$ to $21'' + 6'''$ the annual precession of the equinoxes in seconds produced by the Sun's attraction.

§ 10.

§ 10. We might now proceed in a similar way to investigate the effects of the Moon's disturbing force, the rotation of the earth's axis, and the equation of the precession; but since these propositions are purely mathematical, and the computations have already been gone through by other authors, it will be needless to repeat them here.

§ 11. NEWTON was the first who attempted to explain the precession of the equinoxes from its causes. Since his time various other solutions have been given us by the most celebrated mathematicians; and it deserves to be noticed, that, in a case where there can be little doubt that he was mistaken, other authors have found it difficult to agree among themselves in differing from him. M. D'ALEMBERT, in the year 1749, printed a treatise expressly on the subject, and has since said ^(a), that himself is acknowledged to be the first who determined rightly the method of solving such problems. EULER, DE LA GRANGE, FRISIUS, SILVABELLE, WALMESLEY, SIMPSON, EMERSON, have each considered the subject, and perhaps the importance of the enquiry would justify a minute examination into the cause of the agreement or disagree-

(a) D'ailleurs, des géomètres, vraiment capables d'apprécier mon travail, ont abondamment suppléé à tout autre témoignage, en déclarant que j'ai ouvert le premier la route pour refondre ce genre de questions. See *Opusc. Math.* vol. V. sur la Précession des Equinoxes.

ment of their several methods; but I am deterred from entering into such a discussion by the length of time which it would require; especially as I think those who have read the authors mentioned will easily conceive the substance of what I should have to observe, and to those who have not read them I should hardly be able to say any thing intelligible.

§ 12. The above solution, if it had no other advantages, is, I apprehend, much more concise than any that has hitherto been given. Abstracted from what is said by way of illustration, articles 4th to 9th contain all the calculation requisite, and as I have studiously avoided the ambiguous use of the terms *force*, *vis*, *efficacia*, *momentum*, &c. as well as every doubtful representation of times, spaces, and velocities, which are often substituted by authors in equations, I believe the whole process will appear easy, and the evidence upon which the conclusion rests be exactly ascertained.

§ 13. The principles described in articles 2. and 4. depend upon the third law of motion, and the property of the lever, and are demonstrated in the following manner. Every thing remaining the same as in art. 2. (fig. 6.) let AV and BR, perpendicular to the right line or axis AE, represent the forces and directions with which those bodies are respectively urged, when at liberty to move
I freely

freely in those directions; and let Av , Br , Cc , represent the accelerative forces of the respective bodies, as altered by their mutual actions upon each other: then, because $C \times Cc$ is the moving force gained by C , and $A \times vV + B \times rR$ the moving force lost by A and B , regard being had to the lengths of the different levers AE , BE , we shall have $A \times vV \times AE + B \times rR \times BE$ equal to $C \times Cc \times CE$, that is, $A \times AE \times Av - A \times vV + B \times BE \times Br - B \times rR$ equal to $C \times Cc \times CE$, and by transposition $A \times AE \times Av + B \times BE \times Br$ equal to $C \times CE \times Cc + A \times AE \times Ar + B \times BE \times Br$. Let s , s' , represent, as in art. 2. the spaces which would be described by the bodies A and B at liberty in any very small portion of time, and let x be the space which A actually describes in that time when connected with B and C by the lever AE . The quantities $\frac{x \times BE}{AE}$, $\frac{x \times CE}{AE}$, will then be the spaces described by B and C respectively; and, lastly, because the spaces described in given times are as the accelerating forces, the

above equation gives x equal to $\frac{A \times AE \times s + B \times BE \times s' \times AE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$.

The same method extends itself easily to more difficult cases, and by its assistance several very important theorems are briefly demonstrated.

§ 14. The reasoning made use of in art. 6. will appear very evident to any one moderately versed in the

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elements

elements of mechanics and the doctrine of moving forces; and therefore I must believe that it is by mistake that one author of note entirely omits so necessary a step which affects the conclusion by just one half. When a body moves with any velocity in the direction AM (fig. 7.) which would carry it through the space AD in a small particle of time, and any force which may be reckoned constant for that time urges the body through the space DC perpendicular to AM , the body at the end of that time will arrive at the point C ; but joining AC we are not to suppose, that, if that force ceased to act, the body would proceed in the direction ACL : for take cm equal and parallel to AD , and cd in CD produced equal to $2CD$, and the direction of c at that point will be cl , the diagonal of the parallelogram $cdlm$.

Thus when a body revolves in any curve by a centripetal force (fig. 8.) we may, with Sir ISAAC NEWTON, suppose the curve to be composed of an indefinite number of right lines, and the body to move either in the chords or the tangents of the curve; but then we are to take care that we make not suppositions inconsistent with each other. Let the curve be a circle, and AD a tangent at the point A the direction of the body's motion when it arrives at that point, and let DC , parallel to the diameter AL be the effect of the centripetal force: then, if we

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suppose

suppose the body to move along the chord AC , and say, that the angle CAD measures the deflection of the path in the time of the body's moving through the arc or chord AC , we shall mistake by one half of the true quantity; for draw the tangent at c , and since Ad is equal to dC from the property of the circle, the angle cDd of deviation is equal to twice the angle CAd . The practice of NEWTON in a similar case, where he is investigating the horary motion of the lunar nodes in a circular orbit, is entirely consistent with this. See the Principia, lib. III. prop. 30.

§ 15. M. D'ALEMBERT has lately charged SIMPSON's account of the precession of the equinoxes with some mistakes of this nature in his second lemma; but, in justice to SIMPSON, I must say, that, whatever other defects there may be in his paper, I am convinced, after the most diligent attention, that those alluded to are without foundation.

§ 16. Sir ISAAC NEWTON first observed, that an homogeneous globe could not possibly retain many distinct motions, without compounding them all into one, and revolving with a simple and uniform motion about an invariable axis. When two forces impress upon a globe two distinct circular motions ^(b), he briefly concludes in

(b) See Principia, lib. I. prop. LXVI. coroll. 22.

his

his way, from the laws of motion, that it is the same thing as if those two forces were at once impressed in the common intersection of the equators of those motions, and upon this principle we supposed Az in art. 7. to be the direction of the new equator. In order to remove any doubts that might arise about the justness of this mode of compounding motion, FRISIUS has given a geometrical demonstration of the principle: but the thing may be shewn much more easily in the following manner. Suppose (fig. 9.) RB , AB , to be two axes about which every point in the plane $ABPR$ tends to move with velocities as the respective distances from the axes; let PQ perpendicular to AB be to PR perpendicular to RB as the angular velocity of P about RB to the angular velocity of the same point about AB , and let the velocities be in contrary directions: then, I say, every point in the plane will move with a velocity proportional to its distance from the axis PB . First, it is evident, that any point c in the axis RB will move round PB with a velocity proportional to its distance cm : for the point c lying in the axis RB has no velocity round RB , and cm is proportional to cn . Draw pc parallel to AB , and any point d in that line will move with a velocity proportional to dv , which is perpendicular to PB , for the following reason.

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The velocity of D is equal to the difference of its two velocities round the respective axes RB, AB, or the difference of D's velocity round RB, and P's velocity round AB, since all the points in PC move with the same velocity round AB. Draw DT parallel to CR, and this difference will be proportional to PT, because the velocities of P round RB, AB, are supposed equal to each other, and PT is proportional to DV, and every point in the plane moves round PB with a velocity proportional to its distance; and the same thing may be shewn when any point is taken without the plane ABCPR.

§ 17. Because any point c in the axis RB moves with the same velocity round PB as it does round AB, the angular velocities round the two axes AB, PB, will be to each other inversely as their respective distances CN, cm; and because $CN : cm :: PB : PC$ and $PR : PQ :: PC : CB$, it follows, *that PB the diagonal of the parallelogram PGBC will represent the angular velocity of the revolving plane, when BG, BC, are taken to each other as the angular velocities of the same plane round those respective axes.*

§ 18. From this it clearly follows, that the reason given by SIMPSON ^(c), in his miscellaneous tracts, of the difference between his own solution and that of NEWTON in the Principia, cannot possibly be the true one. “ It

(c) Pages 44. and 45.

“ appears

“ appears further,” says he, “ by perusing his thirty-ninth
 “ proposition, that he there assumes it as a principle, that
 “ if a ring encompassing the earth at its equator, but de-
 “ tached therefrom, was to tend or begin to move about
 “ its diameter with the same accelerative force or angular
 “ celerity as that whereby the earth itself tends to move
 “ about the same diameter through the action of the
 “ Sun, that then the motion of the nodes of the ring and
 “ of the equator would be exactly the same.”

The principle is certainly implied in NEWTON's proof, and is capable of the most rigid demonstration, art. 16, 17.

§ 19. It will be asked then, where is the fault of NEWTON's reasoning? How comes his conclusion to be too little by above one half? It is acknowledged on all hands that there is an error in his third lemma; but then the correction of that error makes only a very small alteration in the result.

It is impossible for any one to form a complete judgement of his method without going through the whole of his calculations, which pre-supposes that the mean motion of the lunar nodes is computed. This motion may be concisely determined and exactly enough for the purpose from prop. 30. of the Principia, and from thence is inferred the motion of the nodes of a satellite revolving in

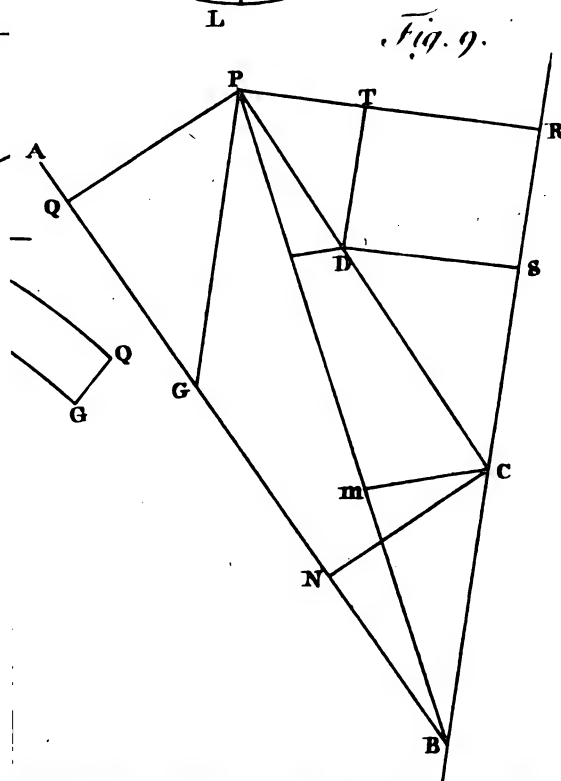
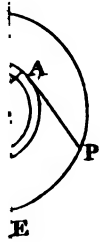
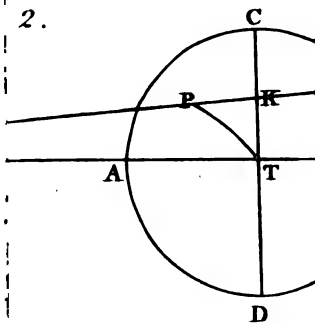
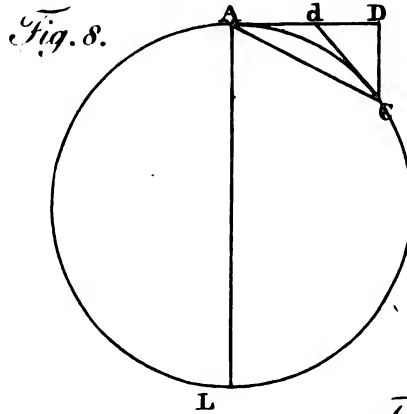
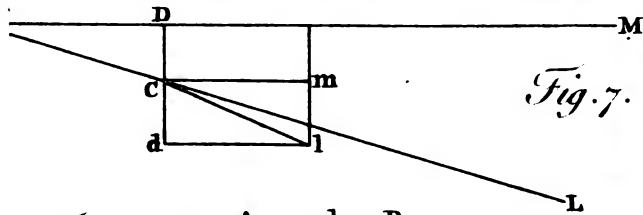
in the plane of the equator at the surface of the earth with a velocity equal to that of the earth round its axis. We are then to suppose, that the *mean* motion of the nodes of a satelite, revolving with such a velocity, is the same with the motion of the nodes of a ring of rigid matter surrounding the earth at its equator, and revolving with the same velocity. This last hypothesis is admitted by SIMPSON, who thinks that his own second lemma contains a full demonstration of the point. For my own part, I believe with FRISIUS, that we are to look here for the material error in NEWTON's solution of the problem. It is evident, that the *true* motion of the nodes of the satelite, and the ring of matter, are not the same; and it is by no means obvious, that their mean motions are so. The mean motion of the nodes of a ring of hard matter cohering together is very easily computed by the method in art. 4th to the 9th, and turns out nearly double the mean motion of a Moon revolving at the surface of the earth with the same velocity.

It is a very interesting enquiry to find out the real cause of the mistake in the Principia, lib. III. prop. 39.; and therefore at a future opportunity I may, perhaps, consider this particular part of the subject more attentively. I have long been satisfied with the account already given, and

should probably have remained so, if M. D'ALEMBERT ^(d) in his Opusc. vol. V. had not persisted to affirm, that the mean motion of the nodes of the ring of matter and of the satellite were the same.

This opinion of so celebrated a mathematician raises scruples in one's mind; and shews, that when we venture to differ from Sir ISAAC NEWTON in these matters, it is with the utmost difficulty that we can arrive at certainty.

(d) Il n'y a de parité que dans le mouvement *moyen* de ces deux anneaux, ou de l'anneau solide et de la lune; les mouvemens *instantanés* sont très différens de part et d'autre; ainsi la comparaison du mouvement de l'anneau avec celui de la lune, serviroit tout au plus à trouver le mouvement *moyen* de l'anneau, ou de la précession des équinoxes, mais nullement à déterminer la nutation de l'axe et l'équation de la précession.



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XXXIII. *An Examination of various Ores in the Museum of Dr. William Hunter. By George Fordyce, M. D. F. R. S. and Mr. Stanefby Alchorne.*

Read May 20, 1779.

MINERS and assay-masters being generally employed in finding out metals for profit, without inquiring into the state or combination they are in, have made use of processes adapted, in many cases, to investigate the quantity of metal, guessing often at its state or combination; and specimens being procured from them for cabinets, an erroneous account has not only been given of their contents, but these errors have also crept into books of natural history: for this reason Dr. FORDYCE determined to assay many of the doubtful specimens, by processes better adapted to find the state or combination of the metal than those commonly in use; and procured the assistance of his friend Mr. ALCHORNE. The result of some of their inquiries they now lay before the Society.

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The first ore we mean to consider is called gold pyrites. Pyrites is an ore of iron, containing sometimes copper, sometimes arsenic, sometimes other metals; generally, if not always, combined with sulphur.

This ore, from its yellow colour, has at first sight been often taken for an ore of gold. It is the most common of all ores, and on examination is very seldom found to contain gold. Specimens, however, have been found from which gold has been procured; and, particularly, there have been found in Fatchobuigna near Zalatna, in Transylvania, masses which contain a large proportion of this metal. Sometimes the gold is in its metallic form, and visible to the naked eye; sometimes it is not: and in these cases the ore has been thought to contain the gold united first with iron, and that compound united with sulphur.

Dr. FORDYCE observes on this proposition, that it has not as yet been proved, that a compound metal can be combined with sulphur. If two metals are soluble in sulphur, and each be separately combined with it, the two compounds may be diffused through one another, as is the case with the compounds of sulphur and iron, and sulphur and copper; which may be diffused through one another: but if we have a compound of two metals, of which one is soluble in sulphur, and the other is not, if

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we apply fulphur to this compound, it will dissolve the one, and leave the other; as, if gold and silver be combined, if we reduce the mass to fine particles, and mix them with fulphur, and throw them into a crucible, heated white hot, and afterwards melt the whole mass, the silver will combine with the fulphur, and the gold will fall to the bottom of the vessel. This being the case, we doubted whether this ore contains the gold in its metallic form, only mechanically mixed with the pyrites; or combined with the fulphur by means of the iron; and therefore subjected a specimen to examination, in which we could not discover, even by the help of a microscope, any particle of native gold.

EXP. 1. We powdered one hundred grains of this ore, and boiled it in nitrous acid diluted with water; a solution took place with effervescence. Having digested them together, till the whole soluble part was taken up, and poured off the solution, and made a precipitation by fixed vegetable alkali, the precipitate appeared to be iron. Having washed the remaining part with water, and exposed it in a glass matrass in sand, to nearly a red heat, a very small portion of sulphur sublimed; the remainder was quartzose sand, with particles of gold, which were similar in figure, though small, to the particles of gold found native in veins mixed with various matrixes, and

not at all like particles which had been combined with a *menstruum*, which ought either to have appeared in a powder, whose particles were hardly visible from their smallness; or in crystals similar to one another.

EXP. II. If any metal be combined with sulphur, mercury will not precipitate the sulphur from it; we therefore took 140 grains of the same ore, and triturated it for some hours with about five times its weight of mercury: the powdered ore was washed off from the mercury, the remainder put into glass vessels, and evaporated by heat: a mass of gold was left, but part of it being accidentally lost, its weight could not be ascertained; it did not amount to above two or three grains at most.

The powder, washed off and dried, weighed 134 grains: these, mixed with as much litharge, and four times their weight of fixed vegetable alkali, and one fifth of wheat flower, and the whole melted produced a regulus of lead, weighing 80 grains, which, on coppelling with a few grains of silver, and parting in *aqua fortis*, left one sixteenth of a troy grain of gold.

It is to be remarked, that in great works, where gold is separated from pieces of crucibles, sand, or other matter, by amalgamation, notwithstanding the process be

frequently repeated, and with great care, some small portion of the gold still remains; so that the small quantity left in this case might easily have either escaped the mercury, or, have been left unobserved in the powder combined with small particles of it.

We may therefore conclude, that in this ore the gold was in its native form, and not mineralized.

EXP. III. We examined gold pyrites mixed with quartz, with a deep magnifier, and found evidently native gold interspersed. One hundred grains of this ore, was powdered; and boiled in nitrous acid diluted with water: a solution of iron took place, as in the first experiment. The *residuum* being exposed to a red heat, there was no appearance of sulphur; the gold was found in such large particles, and so similar to native gold, that we did not think it worth while to apply mercury.

The gold being dissolved out by *aqua regia*, a quantity of quartz and arsenical salt was left.

EXP. IV. When gold and silver are found in their metallic form, it is not uncommon to find them mixed; but it seldom occurs, that they are mixed in so large proportion as in an ore obtained from Norway, and which was given to Dr. HUNTER, by Mr. FABRICIUS.

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The ore has the appearance of native silver, in scaly particles, intermixed with a hard quartz, tinged brown in some parts by iron. Twenty-five grains of the mass, where the metal was in the largest proportion, melted with litharge, alkali, and phlogistic matter, and afterwards coppelled, yielded a globule apparently silver weighing two grains which, being boiled in nitrous acid diluted with water, left full nine sixteenths of a grain of fine gold. Hence one hundred pounds of metal obtained from this ore, consists of seventy-two pounds of silver, and twenty-eight pounds of gold.

There is an ore of silver which is commonly called vitreous (*minera argenti vitrea*, or *argentum vitreum*).

This ore has always been supposed to consist of sulphur and silver; because, if we melt sulphur and silver together, they form a mass which resembles it, especially in colour and malleability; but, as we could find no experiment in any author which authorized this conjecture, we determined to endeavour to ascertain it by analysis, Dr. HUNTER not refusing to subject the ore to an assay, although scarce and expensive.

EXP. V. Fifty grains of this ore broken in pieces, for it is too malleable to be powdered, were boiled in nitrous acid diluted with water; the acid dissolved the silver with much difficulty, and left a *residuum*. The solution being
poured

poured off, and the *residuum* washed and exposed in a glass matrafs to a red heat, there was no sublimate of sulphur, or any thing else obtained. The *residuum*, before it was exposed to heat, did not appear to contain any sulphur; being of a blackish colour after exposure to heat, it had a number of yellow particles mixed, which easily dissolved in *aqua regia*. Tin being applied to the solution made no precipitation, which it would have done if those particles had been gold; hence we supposed they were iron.

The silver was precipitated from the acid by copper: washed and dried it weighed four grains, which, on cupping with lead, lost one twenty-fourth.

There were particles of powdered quartz apparently mixed with the yellow particles left after the solution.

EXP. VI. Being much disappointed in not finding any sulphur in the former experiment, we took three hundred grains of the same ore. We freed it as well as possible from heterogeneous matter: we mixed it with four times its weight of mild fixed vegetable alkali, put them into an earthen body, to which was affixed a glass head, and exposed them to nearly a white heat in a naked fire for a full hour and an half; but no sublimation took place, there distilling only a few drops of water. We then put the body into a melting furnace, and rendered

the whole fluid; as soon as it was melted, it was removed from the fire.

The mass, on concreting, was found divided into two; a black mass a-top, and a metallic mass at bottom. The metal being assayed by solution in nitrous acid and precipitation with volatile alkali, shewed no sign either of gold or copper; lost nothing by coppellation with lead; it weighed 213 grains, which were pure silver.

The black mass a-top; or *scoria*, was boiled repeatedly in water, but did not all dissolve. The insoluble part was unfortunately thrown away; but to the solution we added muriatic acid: on the addition of the acid, there was a strong smell of *hepar sulphuris*, and a copious precipitate, which, on being examined by a microscope, appeared to consist of pellucid crystals, without the smallest appearance of sulphur. This precipitation, being exposed to heat, did not smell in the least like sulphur; it was not in the least inflammable. Excepting then the smell of *hepar sulphuris*, there does not appear any mark of sulphur in this ore, and a very small particle of inflammable matter dropping in by accident would give this smell.

The foregoing experiments occasioning some doubt of sulphur's being contained in vitreous silver ore, we endeavoured to investigate it by other means; and after several experiments,

experiments, we made the following one, which seems conclusive.

Half an ounce of silver was precipitated from nitrous acid by copper, in small flaky crystals as usual; being washed and dried, it was mixed with the same weight of sulphur, and put into a crucible, over which another was placed so as to cover it, and the two crucibles were luted together, leaving a small aperture for the escape of vapour, but not so as to admit air for the inflammation of the sulphur. These being put into the fire, as soon as they were heated red-hot, the sulphur in part escaped through the small hole, and as it passed through burnt with a blue flame. The fire was increased to a sufficient degree to melt the mass within; in the meantime the blue flame disappeared, which shewed that no more sulphur escaped.

After we had applied heat enough, as was supposed, to melt the mass, the crucibles were removed from the fire, and being separated when cold, a regular button-like mass was found in the crucible, of a dark lead colour both without and within, brittle, and streaked on the inside, bearing to be cut with a knife, exactly like the vitreous ore, which it in every way resembled. The whole mass gained twenty grains or one twelfth in weight, which may be considered as the true weight of the sul-

phur in this compound, as silver combines with sulphur in its metallic form, without losing its inflammable air. Fifty Troy grains of this compound was powdered with difficulty, from its toughness, and boiled in nitrous acid diluted with water; it dissolved with difficulty, leaving a very small quantity of a light blackish powder, certainly not nearly a grain in weight.

It appears, therefore, that the small portion of sulphur contained in the vitreous ore may have been decomposed by the nitrous acid, and the alkali, or the heat, in the former processes; and we may conclude, that vitreous silver ore is a compound of silver and sulphur, and when pure, that it contains between ninety-two and ninety-three grains of silver in one hundred.

XXXIV. *On some new Methods of suspending Magnetical Needles.* By John Ingen Housz, *Body Physician to their Imperial Majesties, and F. R. S.*

Read June 17, 1779.

THE very great utility which navigation derives from the use of compasses, has been the principal reason, that so much labour has been bestowed by many ingenious men in searching the best method of suspending magnetical needles, and that some Academies of Sciences have proposed considerable premiums to be given to him who should succeed the best in this important object.

It has been, and is still, a general complaint, that such needles as were by their size and figure susceptible of the greatest magnetical power, and which were executed with the greatest exactness, and perfectly balanced, were, for this very reason of their superior accuracy, so easily put in motion, that the slightest shaking of the floor, occasioned by the observers walking in the room, communicates to them such a great quivering motion as to drag them out of their direction, and to render a considerable
time

time requisite before they are again fixed in the true magnetical meridian.

Since the late Dr. KNIGHT has improved so greatly the method of communicating a considerable magnetical force to steel bars, needles have been impregnated with a much greater and more permanent polarity than it was possible to give them before, and the construction itself of compasses has been considerably improved. But the subject seems not yet to be exhausted.

The degree of magnetical power or polarity to be communicated to steel well hardened seems to depend in some respect on the proportion which the surface of the piece of steel to be impregnated bears to its bulk; so that a thin lamella of steel, weighing for instance ten grains, will acquire more magnetical force than a little lump of steel of the same weight. Thus a small magnet, composed of a great many thin lamina of steel pressed together, may be made so strong as to support 150 times its own weight, and even more, which has never yet been done by a compound heavy magnet.

If the too great quivering and horizontal motion, or the too great restlessness of a strong magnetical needle, is in some way or other counteracted by the methods hitherto adopted, the needle is in some danger of stopping near the true magnetical meridian, without always pointing

pointing directly in it; because near this very place the power of direction of the needle may be so weak as not to overcome the resistance, which was opposed to its free horizontal circular motion.

I thought, in the course of last year, that a great part of the above mentioned difficulty might be taken away by different methods, some of which are the following.

EXP. I. I placed an ordinary magnetical needle, supported upon its point, in a china basin, and poured water into it, so as to cover the whole needle. The needle lost a great deal of its restlessness, was influenced by the approach of a magnet at a considerable distance, and seemed to point as well as before to the magnetical meridian, though with a slower motion.

EXP. II. I took a strong flat magnetical needle, having in its center a round hole, but no cap. I fixed to this needle as much cork as was necessary to make it just swim upon the water in a basin. I fixed a smooth brass pin in a vertical position, so that it passed through the hole in the center of the needle, to prevent its swimming to the sides of the basin. This did tolerably well: the needle moved to the magnetical meridian with a slow motion.

EXP. III. I then took the needle used in the first experiment, and fixed upon it as much cork as was requisite: just

just to make it sink. I placed a point under it, so that the needle with the cork was kept a little below the surface of the water, bearing upon the point with a very considerable weight. This answered nearly as in *EXP. II.*

EXP. IV. I fixed to the center of a small, but strong, magnetical needle a silver wire; to the other extremity of this wire I fixed a small steel-working needle, very much hardened. I stuck the point of this working needle to the lower extremity of a steel magnet, placed in a vertical position and highly polished. This whole apparatus was placed in a glass cylindrical vessel, filled with water so far as to cover the point of the working needle. The horizontal magnetical needle, thus suspended, was remarkably quick in directing itself into the magnetical meridian, and was very easily displaced out of its direction by the approach of a magnet at a considerable distance.

The greatest difficulty I found in this contrivance was, that a little jerk shook the point of the working needle from the vertical magnet; and that a heavy magnet could not be suspended from the point of a working needle; but required a tolerably good magnet rounded at its extremity, where it was suspended from the vertical magnet, or a thick piece of iron rounded in the same manner, by which means, however, the horizontal magnet

magnet did not move so freely. The first difficulty could be easily diminished by letting the working needle pass through a small and smooth hole in a glass or metallic plate, to controul its vibrating motion, and by adapting to it a shoulder, by which it should stop upon the glass or metallic plate, and from which it should be taken up again, by sliding the vertical magnet a little down so as to touch the point of the working needle, and to lift it up again into its former place. The second difficulty could, I think, also be in a great measure obviated by employing, instead of a common magnetical needle, a thin steel tube (of which I will speak by and by) which is only a little specifically heavier than an equal bulk of the liquid. By this means a small vertical magnet could be made to support a large one, even of a considerable absolute weight, of which weight, however, the vertical magnet should support almost nothing, it being counter-balanced by the fluid in which it is immersed.

In the experiment, as I had made it, I could not perceive, that the horizontal magnetical needle was influenced by the vertical magnet, which would probably be the case if the vertical one was much out of the vertical line, and too near the horizontal needle.

EXP. V. I adapted to a flat strong magnetical needle two caps, turned one against another, so that the needle

could be supported on either of the caps, and turned with either surface upwards. I fixed on one of the flat surfaces, on each side of the center, a thin glass tube, hermetically sealed, of such a size, as made the whole together dip in the water so deep that only a small part of the glass tubes remained above the surface of the water. I then depressed the needle entirely under the surface of the water by thrusting a metallic point in the cap which was uppermost, and fixing this point to the cover of the basin: thus the needle, kept under water, bore against the point only with that small degree of pressure which the very inconsiderable difference of specific gravity, which it had above an equal bulk of the water, could give it, which did not amount to above a few grains of weight. Thus this needle, losing nothing of its polarity, lost very near the whole of the resistance of its weight, and at the same time that quivering motion and restlessness, which it had in the air, moving smoothly in a medium, which could only obstruct its too great vibrations (almost in the same way as astronomers stop the vibrations of their plumb line by hanging the weight in water or oil) without obstructing (but retarding only) its tendency to the magnetic meridian, liquids pressing equally on all sides.

To

To prevent this needle from being shook off the point, and wandering about in the basin, I placed also a metallic point underneath, upon which the under cap must bear if the needle should by any cause descend from the upper point, or if I should chuse to make the whole sink by increasing its weight.

Having now found, by experience, that a strong magnetical needle did point to the magnetical meridian near as well under water as in the open air, and that by the resistance of the medium much of its too great versatility was taken away; I wanted to try, what degree of magnetism could be given to a thin steel cylindrical tube, as a needle made in this shape could be as light as required without being incumbered with cork or glass tubes; but finding no such tubes ready made, I tried one made of tin and found it susceptible of a much greater magnetic virtue than I expected, considering that the iron plates of which it is made are neither sufficiently hardened for this purpose, nor approach enough to the nature of steel; besides that, being covered with a pewter coating, they cannot be exposed to the bare friction of a steel magnet.

This experiment, however, afforded me a certain proof, that a magnetic needle, made of a thin steel tube,

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would

would be susceptible of a strong polarity, so as to serve for the purpose here intended.

But as the two agate caps, if fixed in the middle of the substance of the tube, would interrupt some part of the continuation of the steel, and thus lessen the magnetic power, the points of suspension would be better foldered or fixed upon the surface of the tube itself, and the agate caps fixed upon the support underneath and above the tube; so that such a needle should be the reverse (in respect to the suspension) of the common needles. Another advantage would be derived from fixing the points of suspension upon the needle itself, *viz.* that by the motion of the fluid in which it swims, it would be less apt to acquire a too strong waving or undulating motion.

If it should be found difficult to make very thin steel tubes properly hardened, a piece of steel could be scooped out so as to constitute the half of a tube; the other half could be made of another similar piece, or perhaps better of thin brass, or any other metal, and they might be foldered together.

It would, perhaps, answer the same purpose if a steel magnet was shut up in a thin tube of glass or some metal.

I should

I should think, that there could be no great difficulty to adapt to these kind of needles a thin glass, metallic, or enamelled plate, serving instead of the card in the common nautical compasses, and to make use of them in azimuthal compasses.

A card, if it is not required to be stuck upon the needle, might be fixed to the bottom of the glass basin destined for this apparatus.

Common water would be, perhaps, the best medium for these different contrivances, if steel was not so easily rusted by it, if the needle was covered with some varnish or metal unalterable in water, and if in cold weather it was not so apt to freeze; and therefore, I think, some of the thinnest expressed oils would answer the purpose better. Linseed oil, when it has subsided, is very clear and transparent, and is not subject to freeze, at least in the common cold of our climates; besides, it increases the magnetical power of steel, as Dr. KNIGHT found. If it thickens by time, it may be changed.

The glass basin containing such a compass should be kept full of the liquid to the cover, to obstruct undulating motions.

The plate joined to this paper is not made to serve as a model of a complete apparatus, as the drawing was made
from :

from fancy. It is destined only to convey an idea of the invention. I make no doubt but that ingenious artists would soon greatly improve this rough sketch, if the contrivance should be found by experience to answer at sea.

XXXV. *Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1778. By Thomas Barker, Esquire. Communicated by Thomas White, Esquire, F. R. S.*

Read May 20, 1779.

		Barometer.			Thermometer.						Rain.
		Higheft	Loweft	Mean.	In the Houfe.			Abroad.			
					High.	Low.	Mean	High.	Low	Mean	
Jan.	Morn.	29,90	28,16	29,27	42½	32	37	41	18½	32	1,980
	Aftern.				43½	32½	38	44½	21	37	
Feb.	Morn.	29,85	28,66	29,33	44½	34	38	45	24	33	0,949
	Aftern.				45	34	39	51	32	38½	
Mar.	Morn.	29,98	28,61	29,32	51	35½	41½	52	24½	36	1,196
	Aftern.				51½	37½	43	59	36	44	
Apr.	Morn.	29,81	28,92	29,31	56	41½	47½	51	30	41	1,037
	Aftern.				58	43	49	65½	40	51	
May	Morn.	29,79	28,94	29,41	61	48½	54	59½	45	50	1,322
	Aftern.				62	49	55½	70	52	61	
June	Morn.	29,86	29,18	29,58	66	53	60	65½	43	55½	2,714
	Aftern.				68	55	61½	80½	57	69	
July	Morn.	29,82	29,02	29,48	70½	59	64½	66	51½	59½	4,103
	Aftern.				73½	61	66½	85	62½	72½	
Aug.	Morn.	30,00	29,10	29,69	67½	56	63	63	42	56	0,391
	Aftern.				70	56	65	76	57	69	
Sept.	Morn.	29,99	28,72	29,54	62½	48	56	55½	32½	46	1,660
	Aftern.				64	50	57	66	44½	58½	
Oct.	Morn.	29,64	28,62	29,31	55	44	48	52	26	39½	4,238
	Aftern.				57	45	49½	61	38½	50	
Nov.	Morn.	29,72	28,63	29,23	52	42	46½	53	30	40½	3,845
	Aftern.				53	43	47	56	38½	46	
Dec.	Morn.	30,23	28,25	29,34	51	40	44	51½	28	40	2,835
	Aftern.				51½	41	45	52½	33	44	
											26,270

This

This summer having been hotter than usual, I here give an abstract of the hottest month in it.

		Barometer.			Thermometer.					
		Highest	Lowest.	Mean.	In the House.			Abroad.		
					High.	Low.	Mean	High.	Low.	Mean
June 22	Morn.	29,82	29,10	29,58	70½	62½	65½	66	54	61
to July 21	Aftern.				73½	63	67½	85	70	75

The year began with frost. The sharpest and longest this winter which lasted about a fortnight; but the snow never got deep, and the frost towards the end was much broken, and the winter was not, on the whole, either severe or wet. Some windy wet weather followed the frost; but it was oftener dark, fair, calm, and cold, and frequently scarce either frost or thaw. The spring seed time was good; at first dark and cold, but a fortnight at the end of March and beginning of April was funny, fine, and warm, and some days quite hot; then it turned cold again, with several very sharp, frosty mornings, and sometimes hail and snow.

Towards the end of April it grew mild and growing again; frequent small showers, and sometimes windy the first half of May. As summer advanced it was drier and hotter; very much so in June and July, being the hottest summer since 1762, if not since 1750. The

ground was much burnt, but not so much as sometimes; for two very heavy thunder showers, June 27 and 28, kept the grafs from entirely failing. But as we had scarce a settled rainy day for half a year, only showers often with thunder, those places where they did not fall were much more burnt than we, which was the case in most of the South and East of England, and I believe to the North and West of us they had more rain than we. Many and heavy showers in the twelve last days of July made the grafs grow again for a while; but the harvest was exceeding fine, not a day's hindrance, many finished in August, and the crops were in general pretty good. At that time the ground burnt again pretty much, there were some showers in September, but the season was in general dry and calm, and it was upon the whole a very pleasant year.

Frosty mornings began early, for there were some before September was out; and soon after the beginning of October it grew wetter, often windy and frequent frosty mornings. This dark, wet season continued till toward the middle of December, and grew more stormy, but fewer frosty mornings after October. In scarce ten weeks there fell near half a year's rain; the dryness of the ground carried it off for a good while, but it was very full of wet at last. The wheat feed-time being early, and

the clays fowed before the ground got wet, it went very well into the ground, and looks finely in most places. The latter part of December was dark, fair, and mild, and in general calm, till the last day a violent storm, which some supposed as strong as that in 1703 did a great deal of damage in the North of England, and the Eastern part of the South of it; but does not seem to have been so strong toward the Western parts.

XXXVI. *Extract of a Meteorological Journal for the Year*
1778, kept at Bristol, by Samuel Farr, M. D.

Read May 20, 1779.

Months.	Barometer.			
	Highest.	Lowest.	Mean.	Vicissitude.
January	30.31	28.54	29.37	— 1. —2
February	30.34	29.25	29.81	— 1. —3 +
March	30.38	29.10	29.77	0.94—1½
April	30.25	29.30	29.78	— 0.30—1 +
May	30.28	29.40	29.56	0.30—½ +
June	30.34	29.65	30.20	0.23—½ +
July	30.29	29.50	29.94	0.24—½ —
August	30.48	29.73	30.15	0.40—1½ +
September	30.40	29.30	29.80	0.77—1 —
October	30.10	29.18	29.70	0.50—½ —
November	30.26	29.12	29.69	0.60—½ +
December	30.74	28.79	29.86	0.86—1½ + rising. — falling.

Months.	Thermometer in.				Thermometer out.			
	Highest	Lowest.	Mean.	Viciff.	Highest	Lowest	Mean.	Viciff.
				° Days				° Days
January	44	31	36	+ 17-1½	46	30	36	+ 12-1½
February	46	35	40	+ 4-½	48	32	40 ⁸ / ₁₆	- 8-½
March	52	39	43	- 9-2½	56	36	44 ⁸ / ₁₆	- 9-½
April	63	43	51	- 6-½	70	41	53	- 10-1½
May	66	50	57	+ 4-½	70	48	58	- 12-½
June	74	55	61	+ 5-½	78	53	64	- 12-½
July	75	61	67	- 4-½	79	57	68½	+ 7-1½
August	73	59	65	+ 7-1½	78	61	68	- 7-1
Septemb.	64	47	57	+ 7-½	67	46	58	- 12-1
October	57	41	49	- 11-1½	59	38	47	- 11-½
Novemb.	55	43	49	- 4-½	57	39	46	+ 7-½
December	54	38	45	- 10-1½	53	28	43 ⁸ / ₁₆	+ 15-1
			51½				52	

An

kept at Bristol for the Year 1778.

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An abridged table of the WINDS, &c. for BRISTOL, for the Year 1778.

	N	E	W	S	NW	SE	NE	SW	Rain.	Fair Days.	Frosty Days.	Thunder, &c.
Jan.		2		1		9	11½	7½	2.199	7	11	20. 21. 22. S.W.
Feb.	1½	1½	½	½	4½	10	3½	6	0.957	8	6	
Mar.	1	1		2		3½	15	8½	1.406	12	5	
Apr.		½			4½		13	12	0.663	16		30. E.
May	½		½	1½	1½	5	4	18	2.716	20		
June		1		2	1	1	5½	19½	0.972	19		{ 15. N.W. 24. E. 26. S.W. 29. N.E.
July	1			1	2½	1½	3	22	3.003	22		20. N.E. 28. 31. S.W.
Aug.		½		1½	1½	4	8	15½	0.292	22	1	
Sept.	½			½	6	5½	11	6½	1.323	20	3	11. S.W. 27. N.E.
Oct.		½	½	1	6	11½	8½	3	3.987	18	11	
Nov.	1½			½	14½	2	1½	10	5.085	12	5	25. N.W.
Dec.	½	1	3	½	9	5	½	11½	3.300	11	6	
	6½	8	4½	12	51	58	85	140	25.903	187	48	

WEATHER FOR THE YEAR 1778.

January. Cloudy but dry till 4th, with a frost; rain on 4th and 5th; frosty till 12th; then rainy, except 16th, till 26th; then a frost till 28th, when it thawed as on 29th; 30th and 31st wet.

February. Dry to the 4th and on 5th; then wet to 9th; dry and frost on 13th to 14th; 16th, 17th, 18th, and 19th, frosty; then wet till 26th; that and 27th had some snow; 28th fair,

March. Fair to the 6th; 7th cloudy; 8th wet; cloudy but dry to the 12th, and dry to the 20th; wet to 25th; dry to 28th; after that wet to the end.

April.

April. Dry to the 6th, and after that to 9th, and then to the 13th, and after that to 16th; 17th and 18th fair; 19th and 20th stormy; then dry and frosty to 27th; 28th cloudy; 29th and 30th wet.

May. The 1st dry; then it was wet to the 9th; dry to the 12; then it rained to 19th; was dry to 22d, and afterwards, except 27th, to the end.

June. Wet, except the 3d, till 6th; 7th wet; 8th dry; 9th wet; then dry to 15th, and after that to 24th; 25th dry; 26th, 27th stormy; 28th dry; 29th stormy; 30th dry.

July. The 1st and 3d wet; 2d dry, and to the 16th; then it was wet the rest of the month.

August. Some rain on 3d, 6th, 14th, 15th, and 30th; the rest was quite dry, and chiefly fair.

September. Was dry to the 7th, and on 8th and 9th; then wet to 13th; dry to 16th; 17th wet; then dry to 24th; 25th dry; 26th and 27th wet; 28th, 29th, a frost; 30th wet.

October. The 1st wet; 2d dry; 3d wet; 4th, 5th, dry; 6th, 7th, 8th, wet; then frosty to 19th; after which, except on the 25th, 26th, 29th, it was very wet.

November. The 2d, 8th, 14th, 15th, 20th, 21st, and 30th, were dry; the rest were all wet.

December. 1st and 5th frosty; then it rained till 25th, excepting 19 and 21st; then a frost to 28th; then wet to the end.



XXXVII. *A Treatise on Rivers and Canals.* By Theod. Aug. Mann, *Member of the Imperial and Royal Academy of Sciences at Bruffels; communicated by Joseph Banks, Esq. P. R. S.*

Read June 24, 1779.

TO JOSEPH BANKS, ESQ. P. R. S.

S I R,

YOUR election to the Presidency of the first literary and scientific Society in the world, to a chair so long and so gloriously occupied by the great NEWTON; joined to the friendship you have been pleased to honour me with since my being first known to you: has encouraged me to send you something of my composition, as the best way of expressing my sincere respect and attachment to you, and my profound veneration for the illustrious Body which has chosen you for its head. Though various circumstances, by carrying me very early into foreign countries, have made me from my youth almost

am.

an alien to my native soil, and put me in a situation which apparently must make me ever remain so; yet, neither time nor distance could ever weaken, much less obliterate, my tender attachment to it, or my ardent wishes for its welfare.

These considerations will, I hope, merit a favourable acceptance from the Royal Society of the following piece, which I have the honour of addressing to you; and an indulgent condescension for its imperfection in every respect, and particularly in point of style. Five and twenty years absence from my native country, and the necessity of conversing during that time in different foreign languages, must unavoidably have filled mine, without my being sensible of it, with idioms and expressions in no wise English.

As to the subject I have undertaken to treat on this occasion, I was guided in the choice thereof by the motive of saying something that might be useful to my native country. The great number of extensive and magnificent canals, which have been cut through almost every part of England of late years, for the use of internal navigation, and which do honour to the public spirit of the nation, merit to be considered in a scientific as well as in a commercial light. Their waters have their laws of motion different in many cases from those of
7 rivers:

rivers: they are liable to many accidents which the others are not, and of a different nature. These accidents do not become sensible till many years after their construction, and are better prevented in time than remedied when they happen. I have long lived in a country famous for its navigable canals, and have been much employed, under the eyes of the government of it, upon that subject. I only mention this to shew, that I have not undertaken to treat a subject to which I am an utter stranger.

There are, moreover, many considerations concerning the laws of motion in rivers and canals in general, the velocity of their currents in proportion to the quantity of their declivity, and the means of ascertaining therefrom the respective heights of the interior parts of continents, which merit the attention of a natural philosopher. I shall venture to offer my thoughts and observations (some of which, I believe, are new) on all these subjects in the ensuing Dissertation, which I submit entirely to the judgment of the Royal Society, and shall esteem myself happy if I succeed in it, so as to be of any use to my country, and to be able to testify, at the same time, my profound respect and veneration to you, DEAR SIR, and to the illustrious body over which you preside.

SECTION I.

Different uses for which canals are made, with an account of the principal authors who have wrote concerning them.

1. Artificial canals are to be considered in a double light; as facilitating commerce by means of internal navigation, and as preventing inundations by carrying off the too great abundance of water from low and flat countries, such as are Holland, Flanders, &c. In these last named countries they serve at once for both purposes; and it is in this double light that I shall consider them in the ensuing discourse. If canals for draining have sluices upon them, particularly at the end whereby they discharge their waters, as is universally the case in the Low Countries, they differ in no wise from navigable canals: if they have nothing to sustain their waters in them, they are to be considered in every respect as rivers or rivulets, and follow the same laws. It must, therefore, be carefully kept in mind, that whenever I mention canals, I mean those only whose waters are kept up by sluices, and never those without them, which I include, without distinction, under the common appellation of rivers; for
they

they are no more than artificial ones. If I mistake not, all the navigable canals in England are of the first fort; that is, have their waters kept up, and let off by sluices. This necessary distinction will take away all ambiguity from what I have to say on canals throughout the following discourse.

2. But that I may fulfil the task I have undertaken, it is necessary first of all to lay down such principles on the nature of rivers and canals in general as have been demonstrated true both by calculation and experience; to the end, that we may deduce from thence the true laws of motion of their waters, and the quantity of declivity of their beds: for this purpose, and because a large volume would hardly suffice to comprise all the demonstrations of these principles, which, consequently, I am obliged to omit in this treatise, it will not be amiss to mention the principal authors who have treated this subject in different ages and countries, in whose works the demonstrations of all the principles I shall lay down may be found, if any one doubts the truth of them. These are the following.

SEXTUS JULIUS FRONTINUS, *de Aquæ-ductibus Urbis Romæ, cum Notis POLENI*, impress. 1722.

JOHN BAPTIST ALEOTTI, Hydrometrician to the Duke of Ferrara, and to Pope CLEMENT the VIIIth.

* DON BENEDICT CASTELLI, Benedictine Abbot, de
Menfurâ Aquarum Currentium.

J. B. BARATTERI, de Architettura d'Acque, lib. VI.
Piacenza, in folio, 1656.

ALEXANDER BELTINZOLI, of Cremona.

NICOLAUS CABEUS, in Libris Meteorum.

GALILEI GALILEO.

* JOH. BAPT. BALIANI, de Motu Liquidorum.

* JOH. BAPT. RICCIOLI, Geographiæ et Hydrographiæ
Reform. libro VI. cc. 29. et 30.

* CLAUDE MILLET DESCHALES, de Fontibus et Fluminibus, à prop. 39. usque ad 56.

VARENNIUS, General Geography, with Dr. JURIN's
and Dr. SHAW's Notes, edit. of 1765, vol. I. from page
295 to page 358.

• Dr. JURIN, in the Philosophical Transactions, N° 355-
page 748. et seq.

MARIOTTE, Traité du Mouvement des Eaux.

VARIGNON, Memoires de l'Academie des Sciences de
Paris, pour 1699 et 1703.

* Sir ISAAC NEWTON, Princip. Mathem. lib. II. § 7.
page 318 et seq. edit. 1726.

* DANIELIS BERNOUILLI, Hydrodynamica, in quarto,
Argentorati, 1738.

* DOMENICHE GUGLIELMINI, della Natura de Fiumi, Bononiæ, 1697, in quarto. Ejusdem de Mensurâ Aquarum fluentium, Bononiæ, in quarto.

* JOH. POLENUS, de Castellis et de Motu Aquæ mixto, Patavii, 1697, 1718, 1723.

* RACCOLTA d'Autori che trattano del Moto dell' Acque, Fiorenza, 1723, 3 vol. quarto, cum fig.

JAC. HERMANNUS, in Phoronomia, cap 10. page 226. et seq.

CHRIST. WOLF, Curs. Mathem. Hydraulicæ, cap. VI. edit. Genevæ, in quarto, 1740.

M. DE BUFFON, sur les Fleuves, dans son Histoire Naturelle, tom. II. p. 38—100. de la 1^{re} edit. en 12mo.

Several Memoirs upon this Subject in the Collection of the Royal Academy of Sciences of Paris, particularly those of M. PITOT, in the volumes for 1730 and 1732.

S'GRAVESANDE, in Elementis Physicæ, tom. I. lib. II. cap. 10.

* R. P. LECCHI S. J. Hydrostatica, Mediolani, 1765. In this excellent work are several pieces by Father BOSCOVICH upon the same subject.

* STATTLER Physica, vol. III. p. 232—286. de cursu Fluminum, ejusque Mensuratione et Directione. Aug. Vindel. 1772, 8 vol. in octavo. This author gives many late observations and experiments on the motion and
measure

measure of currents, as those of ZENDRINI, HIMENII, &c.

Two other authors have lately wrote upon rivers and canals, but their works are not yet come to my hands; to wit,

Father FRISI, an Italian Barnabite, Professer of Mathematics at Milan.

M. DE LA LANDE, of the Royal Academy of Sciences at Paris, who has just published a History in folio, with plates, of all the Navigable Canals in the World that have come to his Knowledge.

Among the above authors, those marked with an asterisk (*) are they who have treated the subject in question with the greatest exactness or most extent; and it is from them chiefly that I shall lay down such principles and laws of action in rivers and canals as regard the subject I have taken in hand. By this means I shall avoid advancing any thing upon so important a matter, but what is founded upon the most certain and exact experiments, and conformable to what are demonstrated to be the real and unalterable laws of nature.

S E C-

SECTION II.

The theory of rivers and canals.

I. DEFINITIONS.

3. A river is a greater or lesser quantity of water which runs constantly, by its own gravity, from the more elevated parts of the earth, towards those which are more depressed, in a natural bed or channel open above.

4. If this bed or channel is artificial, and has been dug by hands, it is called *a canal*, of which there are two kinds; those where the channel is every where open, and without sluices, which I call an *artificial river*; and those where the waters are kept up or let off by the means of sluices: it is this second sort which I shall call henceforwards by the proper name of *a canal*.

5. *A river is said to persevere in the same state* so long as there runs off an equal quantity of water in the same time, without any increase or diminution, so that it remains always at the same height in the same place. When the circumstances are different from this, it is said respectively that *a river increases or diminishes*.

6. *A section* of the bed of a river or canal is a plane drawn perpendicular to the bottom of the bed and to the direction.

direction of the stream of water, and whose limits are those of the water itself which runs off in that place.

7. I call *sections of equal velocity*, all those where the water runs with equal velocity; and *sections of greater or lesser velocity*, those where the water runs faster or slower respectively, and when compared to others.

8. I call *mean velocity of a current or stream of water*, that which a river or canal would have, if all the parts thereof, to wit, those of the bottom, the sides, the middle, and the surface of the same section, ran with an equal velocity, in such a manner that there would pass just as much water in the same time by this uniform motion, as there does now actually pass by the irregular flowing of the stream.

II. PROPOSITIONS, or laws of action in rivers and canals.

9. The motion of water in rivers proceeds from the same principle which produces the descent of heavy bodies upon inclined planes.

11. The descent of heavy bodies upon inclined planes follows exactly the same laws as those observed in the descent of heavy bodies in a perpendicular line towards the center of the earth; that is,

1st, They descend by a motion uniformly accelerated.

2dly, The

2dly, The spaces, run over by heavy bodies which fall perpendicularly by a motion uniformly accelerated, are in a *duplicate ratio of the times and velocities* respectively.

3dly, These spaces, in equal times, augment in the same ratio as the odd numbers in progression 1, 3, 5, 7, 9, 11, &c.

4thly, Therefore, both the times and the velocities are in a *sub-duplicate ratio of the spaces run over*.

5thly, It is demonstrated by the principles of mechanics, that the velocity acquired by a heavy body descending freely upon an inclined plane, in a given time, is to the velocity which the body would acquire in the same time, by falling perpendicularly, as the height of the inclined plane is to its length.

6thly, From whence it follows, that the velocities which bodies acquire in their descent upon inclined planes, are in a *direct ratio of the square roots of the quantity of inclination or declivity of the planes*.

11. So that when water flows freely upon an inclined bed, it acquires a velocity, which is always as the square root of the quantity of declivity of the bed.

12. In an horizontal bed, opened by sluices or otherwise, at one or both ends, the water flows out by its gravity alone; and the flowing is quicker or slower in a di-

rect ratio of the respective heights of the water, by reason of the weight of the superior waters upon the inferior.

13. From hence (N^o. 11, 12.) it follows, first, that as much as the declivity of the bed or channel of a river is greater, so much also will the velocity of the flowing waters be proportionably increased.

2dly, As much as the water in an horizontal bed is deeper, so much will the velocity of the current be increased; and this velocity will diminish in proportion to the decreasing depths of the water in the bed.

3dly, Abstracting from the resistance caused by the bottom and sides of the bed, as much nearer, as the water is to the bottom, so much will its motion be accelerated; not only because the inferior waters are more compressed by the superior, in proportion to their greater depth; but also because the inferior ones have a greater declivity, than the superior, by reason of their greater depth in the bed, where they are more depressed with respect to the elevation of their common source or spring. But these different velocities of the upper and lower waters in the same section of the bed (abstracting from the friction of the bottom and sides) approximate indefinitely to each other in proportion to the length of the channel, but still without a possibility of their ever becoming equal in fact, if they met with no resistance from the bed.

14. There-

14. Therefore, the motion of water flowing freely in an inclined channel, is accelerated by its own weight combined with the quantity of declivity in the bed.

Nevertheless, the velocity of waters which flow in an inclined bed, during their actual flowing, is not accelerated by the weight which the inferior waters sustain from the superior ones, in case the lower parts have already, by the declivity of the bed, a greater velocity than that which the weight of the superior ones impresses upon them. The reason of which is, that no body which follows another with a lesser velocity can act by impulsion upon that which precedes it with a greater velocity, as is the case with regard to these superior and inferior waters. But the weight of the upper waters begins to accelerate the lower as soon as they fall into an horizontal bed; or one

trive excess of the one above the other. It often happens in the same section of a river, that the acceleration of velocity in the inferior parts proceeds from the weight of

the water above. 4 B 2

the upper waters, while that in the upper parts proceeds from the declivity of the bed.

From whence it follows, that in rivers which have little declivity, it is the depth of the waters which contributes most to accelerate their current; and in those whose beds have most declivity, it is the descent of gravity upon an inclined plane which has the greatest share in producing this acceleration.

To find whether the water in a part of a river where the bed is nearly horizontal flows by the velocity acquired in the preceding declivities, or by the compression of the upper waters upon the lower in that place; a pole must be thrust down to the bottom, and held perpendicular to the current of the water, with its upper end above the surface: if the water swells and rises immediately against the pole, it shews that its flowing is by virtue of a preceding declivity: if, on the contrary, the water stops for some moments before it begins to rise against the pole, it is a proof that it flows by means of the compression of the upper waters upon the lower.

16. The *absolute height or elevation* of the surface of a river which perseveres in the same state (N^o 5.) continually decreases, as the distance in the river from its source increases; by reason that its bed must continually incline and tend towards the center of the earth.

17. The

17. The velocity of each particle of water in a regular channel, that is, where the bed is a regularly inclined plane, may be determined by drawing a perpendicular from the particle proposed to the horizontal curve which passes through the spring, or that point of the river where the particle in question begins to acquire its velocity. For the velocity which this particle would acquire, in falling freely along the said perpendicular, is the same as that which it has acquired in its descent along the inclined plane of its bed.

18. So long as *a river perseveres in the same state* (N° 5.) there flows an equal quantity of water in equal times, how unequal soever the sections be through which they flow; and, consequently, where the section of the river is greater, the velocity of the flowing water is less; and where the section is less, the velocity is greater; always in an inverse proportion. From hence may be deduced the following and other similar propositions.

1st, Through equal sections, in equal times, and with equal velocity, there must flow equal quantities of water.

2^{dly}, Through equal sections, in equal times, but with unequal velocities, the quantities of water which flow, are in a direct ratio of the respective velocities:

3^{dly}, Through

3dly, Through unequal sections, in equal times, and with equal velocities, the quantities of water which pass are in a direct ratio of their respective sections.

4thly, Through unequal sections, and with unequal velocities, the quantities of water which flow in equal times, are in a combined ratio of the sections and mean velocities (N^o 8.) together.

In a word, the sections of the bed, the mean velocities, the times of flowing, and the quantity which flows, are universally in a combined ratio together; and this combination is what is called *the momentum of a river*; and this *momentum of the same flowing water is universally equal*.

19. From hence may easily be deduced the principles for calculating the quantity of diminution of the water in a lake, pond, or vessel, by any determinate flowings whatsoever: for as the surface of the lake, &c. is to the section of the current which carries off the waters; so is the mean velocity of the current in this section to the decrease of the waters in the lake, &c. and vice versa.

III. On

III. On the nature of rivers and flowing waters.

20. Rivers contain divers inherent causes of the acceleration of their motion.

1st, Their springs are either in mountains or on high grounds, and it is by the descent of the waters from these elevations that they acquire a velocity and acceleration of motion sufficient to sustain and propagate it through the rest of their course.

2dly, The cohesion of the particles of a fluid, *in a bed ever so little inclined*, is a second cause of acceleration of motion in the fluid; because, by their mutual attraction, those particles which begin first to flow draw after them those which are contiguous, these the following, and so on *ad infinitum*.

3dly, Moreover, where a river, by flowing in a bed nearly horizontal, has lost a great part of the velocity which it had acquired in the preceding declivities, and the bed by this means is become large and shallow, which consequently again augments the slowness of the current; it may, however, recover a part of its velocity, even in the same horizontal bed, by augmenting the depth thereof, and diminishing its breadth; for by this means the weight of the superior waters upon the infe-

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rior is increased, and consequently the velocity of the whole is augmented (N° 13. 18.). In the same manner a junction of rivers in the same bed, by excavating and deepening it, augment the velocity of the common current, as we shall shew more particularly hereafter.

21. On the other hand, flowing waters meet with many powerful causes of resistance to their motion, which tend continually to diminish their velocity. Such are the following:

1st, The attraction and continual friction of the bottom and the sides of the bed, contribute greatly towards retarding the motion of the water.

2dly, The same effect is produced likewise by the many obstacles which they meet with in their way; such as inequalities in the bottom and sides of the channel, banks of sand and mud, rocks, trunks of trees, and other such things.

3dly, The many windings and angles made in their course, which produce so much the more resistance and hindrance to the motion of the water, as the course varies more and oftener from a right-line.

4thly, The diminution of their declivity the farther they recede from their springs; this being generally the least towards their mouths, which are for the most part in extensive plains.

Finally,

Finally, the natural cohesion of the particles of water *in an horizontal bed* contributes to retard its motion precisely by the same force which contributes to accelerate it *in an inclined bed*. By diminishing or taking away the above obstacles to the free motion of water in rivers and canals, the velocity of their currents will be increased in the same proportion, and thereby also all the dangers and ravages of inundations may be prevented, as we shall shew hereafter.

But if some or all of these causes, in a greater or lesser degree, did not exist in rivers of considerable depth and declivity, it is demonstrated^(a), that the velocity of their currents would be accelerated to twelve, fifteen, and, in some cases, even to twenty times more than it is at present in the same rivers, whereby they would become absolutely unnavigable.

22. The waters in a river or open canal have their motion accelerated, so long as the effects proceeding from gravitation, declivity, depth, in a word, so long as *the sum of accelerations surpasses the sum of resistances*.

When these different sums become equal to each other, the motion of the water is neither accelerated nor retarded, but remains equal, till something anew destroys the *equilibrium*.

(a) Vide LECCHI, Hydrostat. et STATTLER, Physic. tom. III. p. 252.

When the sum of resistances and causes of retardation is greater than the sum of accelerating causes, the velocity of the river is diminished in proportion to the excess.

23. The *percussion* of the waters of a river against an obstacle which is opposed to their motion, is the action of the waters striking against that obstacle; and the principles for calculating the quantity of this percussion, or the effects which any obstacles whatever produce in the motion of rivers, by known forces, and in determinate times, are as follows:

1st, The percussion of the water of a river against any obstacle whatever is universally in a compound ratio of the quantity of the plane or planes which the obstacle opposes to the current of water, of the sine of the angle of incidence which the direction of the current makes with these planes, and of the square of the velocity of the said current.

2dly, The resistance, therefore, which the bed of a river opposes to its current from any particular obstacles in it, is in a compound ratio of the magnitude and situation of the planes of these obstacles, together with the square of the current's velocity in the place where those obstacles are found.

3dly, The accelerating force of a river, or that by which it surmounts the resistance of its bed in any one
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place,

place, compared to that in another, is in a compound ratio of the mass of water and of the velocity of the current in those places respectively.

24. In different parts of the same river, the velocity of the current is greater in a direct proportion of the greater declivity of the bed; because the relative gravity of the flowing particles augments in that ratio.

25. But in the same section of a river, the superior parts, and those which are farthest from the bottom and the sides, will continue their course by the sole cause of the declivity of their bed, *how little soever it be*; because these waters not being retarded by the friction of the bottom and sides of the bed, or hardly by any other obstacle whatever, *the least possible deviation from a level will produce a current*. But the waters at the bottom of a river, both because of their friction against it, and of the irregularities which are almost every where found in it, will lose that little motion which a very small declivity can give them, and their motion in that case will be produced alone by the compression of the superior waters upon them.

The inferior waters which thus acquire their motion from the weight of the superior ones upon them, communicate reciprocally a part of their motion, by means of the natural cohesion of the particles together (N° 20.)

to the superior ones, which in an horizontal bed, without this cause, would have no other motion than that which is impressed upon them by the impulsive force of the waters descending from their elevated springs.

From whence it appears, that the superior and inferior waters in a river communicate reciprocally a part of their motion to each other; but this can never go beyond a certain point or *maximum*, which is always proportionable to the *momentum* of the river in that place (N° 18.).

It follows from hence, that the greatest velocity of a river, running in a right-line, is in *the center of its section* (N° 6.); that is to say, in that point which is the farthest possible from the surface of the water and from the bottom and sides of the bed, all taken together. This part has the advantage of one half of the depth of water pressing upon it, and it is exempt from the friction of the bottom and sides of the bed which are there overcome and vanish by the perpendicular compression.

On the contrary, the least velocity of the water is at the bottom and sides of the bed, because it is there that the resistance produced by friction is greatest, from whence it is communicated to the other parts of the section in *an inverse duplicate proportion of the distances from the bottom and sides combined together*, until it becomes a
negative

negative quantity, where the effect vanishes, or is reduced to nothing.

26. The best and most simple method of measuring the velocity of the current of a river or open canal, that I know of, is the following:

Take a cylindrical piece of dry, light wood, and of a length something less than the depth of the water in the river: round one end of it let there be suspended as many small weights as may be necessary to keep up the cylinder in a perpendicular situation in the water, and in such a manner that the other end of it may just appear above the surface of the water. Fix to the center of that end which appears above water a small and straight rod, precisely in the direction of the cylinder's axis; to the end, that when the instrument is suspended in the water, the deviations of the rod from a perpendicularity to the surface of it may indicate which end of the cylinder advances the fastest, whereby may be discovered the different velocities of the water at different depths; for if the rod inclines forwards according to the direction of the current, it is a proof that the surface of the water has the greatest velocity; but if it inclines back, it shews that the swiftest current is at the bottom; if it remains perpendicular, it is a sign that the velocities at the surface and bottom are equal.

This

This instrument being placed in the current of a river or canal receives all the percussions of the water throughout the whole depth, and will have an equal velocity with that of the whole current *from the surface to the bottom* at the place where it is put in, and by that means may be found, both with ease and exactness, the mean velocity of that part of the river for any determinate distance and time.

But to obtain the mean velocity of the whole section of the river, the instrument must be put successively both in the middle and towards the sides, because the velocities at those places are often very different from each other. Having by this means found the *difference of time required for the currents to run over an equal space; or, the different distances run over in equal times, the mean proportional* of all these trials, which is found by dividing the common sum of them all by the number of trials, *will be the mean velocity of the river or canal.*

If it be required to find the velocity of the current only at the surface, or at the middle, or at the bottom, a sphere of wood, of such a weight as will remain suspended in equilibrium with the water at the surface or depth which we want to measure, will be better for the purpose than a cylinder, because it is only affected by the
water

water of that sole part of the current where it remains suspended.

It is very easy to guide both the cylinder and the globe in that part which we want to measure, by means of two threads or small cords, which two persons must hold and direct, one on each side the river; taking care at the same time neither to retard nor accelerate the motion of the instrument.

Several other methods have been invented for determining the velocity of the currents of rivers and canals, which may be seen in most of the authors enumerated in the beginning of this essay (N^o 2.)

IV. Application of the preceding laws of the acceleration and retardation of currents to rivers and canals in general, from whence are deduced the various means of preventing or remedying the defects and inconveniencies which must necessarily happen to them in a series of years.

27. By combining together all we have said hitherto upon the nature and theory of motion in rivers, and particularly in the articles 13. 18. 20. 21. and 23. it follows evidently, that *the deeper the waters are in their bed in proportion to its breadth, the more their motion is accelerated;*

celerated; so that *their velocity increases in an inverse ratio of the breadth of the bed, and also of the greatness of the section*; from whence are deduced the two following universal practical rules:

1st, To augment the velocity of water in a river or canal, without augmenting the declivity of the bed, we must *increase the depth and diminish the breadth of its bed.*

2dly, But to diminish the velocity of water in a river or canal, we must, on the contrary, *increase the breadth and diminish the depth of its bed.*

The above proposition is perfectly conformable to observation and experience; for it is constantly seen, that the current is the swiftest where the waters are deepest and the breadth of the bed the least; and that they flow slowest where their depth is the least and the breadth of the bed the greatest. “The velocity of waters,” says M. DE BUFFON ^(b), “augments in the same proportion as the section of the channel through which they pass diminishes, *the force of impulsion from the back-waters being supposed always the same.* Nothing,” continues he, “produces so great a diminution in the swiftness of a current as its growing shallow; and, on the contrary, the increase of the volume of water augments its

(b) Hist. Nat. tom. II. p. 53. 60. edit. in 12mo.

“velocity more than any other cause whatever.” The celebrated WOLF, in his *Hydraulics*^(c), assures us, that “it is a constant and universal practice, for accelerating the current of waters, to deepen the bed, and at the same time to render it narrower.”

28. When the velocity which a river has acquired by the elevation of its springs and the impulse of the back-water, is at last totally destroyed by the different causes of resistance which we have enumerated above (N° 21.) becoming equal or greater than the first, the bed and current at the same time being exactly horizontal, nothing else remains to propagate the motion, except *the sole perpendicular compression of the upper waters upon the lower, which is always in a direct ratio of their depth.* But this necessary resource, this remaining cause of motion in rivers, augments in proportion as all the other diminish, and as the want of it increases: for as the waters of rivers in extensive plains lose the acceleration of motion acquired in their descent from their springs, their quantity accumulates in the same bed by the junction of several streams together, and their depth increases in consequence thereof. This junction and successive accumulation of many streams in the same bed, which we see universally in a greater or less degree in all rivers throughout the known world, and which is so absolutely

(c) N. 224.

necessary to the motion of their waters, can only be attributed, says Signor GUGLIELMINI ^(d), to the infinite wisdom of the supreme Author of Nature.

29. The velocity of flowing waters is very far from being in proportion to the quantity of declivity in their bed: if it was, a river whose declivity is uniform and double to that of another, ought only to run with double the swiftness when compared to it; but in effect it is found to have a much greater, and its rapidity, instead of being only double, will be triple, quadruple, and sometimes even more: for its velocity depends much more on the quantity and depth of the water, and on the compression of the upper waters on the lower, than on the declivity of the bed. Consequently, whenever the bed of a river or canal is to be dug, the declivity must not be distributed equally throughout the whole length; but, to give a swifter current to the water, the declivity must be made much greater in the beginning of its course than towards the end where it disembogues itself, and where the declivity must be almost insensible, as we see is the case in all natural rivers; for when they approach near the sea their declivity is little or nothing, yet they flow with a rapidity which is so much greater, as they contain a greater volume of water; so that in

(d) *Della Natura de Fiumi.*

great

great rivers, although a large extent of their bed next the sea should be absolutely horizontal, and without any declivity at all, yet their waters do not cease to flow, and to flow even with great rapidity, both from the impulsion of the back waters, and from the compression of the upper waters upon the lower in the same section.

30. Whoever is well acquainted with the principles of the higher geometry, will easily perceive that it would be no difficult matter so to dig the bed of a canal or river, that *the velocity of the current should be every where equal*. It would be only giving it the form of a curve along which a moving body should recede from a given point, and *describe spaces every where proportional to the times*, allowance being made therein for the quantity of effect of the compression of the upper waters upon the lower. This curve is what is called the *Horizontal Isocronic*, being the flattest of an infinity of others which would equally answer the problem where fluids were not concerned. Upon these curves may be seen LEIBNITZ, HUYGHENS, and the two BERNOUILLI's, who were the first that determined and analysed them, and also many succeeding geometricians, if any one is desirous to occupy himself in such speculations as are more curious than useful, which is not my purpose in this treatise.

31. Notwithstanding all we have said concerning the necessity of augmenting the depth of a river in a greater proportion than its breadth, if we would accelerate its current; yet it is certain, that this can only be done to a certain point, without destroying that equilibrium which ought to reign between the depth and the breadth of the section of the stream, and thereby putting the river into a state of continual violence, which will incessantly exert itself to the destruction of the banks and wiers made to keep it in, and that action will always exert itself in a direct ratio of the greater or less want of equilibrium, as it would be easy to demonstrate by the principles of hydraulics. These same principles give likewise the just proportions of this equilibrium between the perpendicular and lateral compression of the water in any river or canal whatsoever, which vary in an inverse proportion, according to the different degrees of the declivity and velocity of the current; and in a direct one of the greater or less coherence and hardness of the substances which compose the bed. Rivers which flow in beds composed of homogeneous matter of little consistency, such as sand, &c. are always more broad than deep, when compared to those which run in beds of matter of greater tenacity. It is manifest, that the equilibrium here spoken of is real,

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because

because rivers remaining in the same state only widen their beds to a certain pitch which they do not surpass.

32. M. DE BUFFON remarks, "That people accustomed to rivers can easily foretell when there is going to be a sudden increase of water in the bed from floods produced by sudden falls of rain in the higher countries through which the rivers pass. This they perceive by a particular motion in the water, which they express in their dialect, by saying that *the river's bottom moves*; that is, the water at the bottom of a channel runs off faster than usual; and this increase of motion at the bottom of the river always announces a sudden increase of water coming down the stream. Nor does their opinion therein," continues the same author, "seem to be ill-grounded on the nature of things; for the motion and weight of the waters coming down, though not yet arrived, must act upon the waters in the lower parts of the river, and communicate by impulsion part of their motion thereto; since a canal or river contained in its bed is to be considered in some degree as a column of water contained in a long tube, where the motion is communicated at once throughout the whole length." In a river or canal, open above, it is only communicated to a certain distance; that is, as far as the impulsive force of the new increase and superior rapidity

rapidity of the back-waters acts upon the stream, which will always be as far as till this force is gradually, and at last wholly, destroyed by the superior gravitation of the super-incumbent waters in the stream. Something of the same kind happens when a very great additional weight comes suddenly upon the surface of a river or canal; for instance, by the launching of a ship or of several boats together upon it. These causes increase the velocity of the water in the lower parts of the bed, and moreover retard its motion at the surface, which effect may properly be called *making the river's bottom move*. For the same reason, the increase of weight of the waters in a sudden flood, as well as the increase of their impulsive force, must contribute to produce this effect, and, by increasing the motion in the bottom of the river, may hinder, for some space of time, the stream from sensibly rising in the bed.

33. All obstacles whatever in the bed of a river or canal, such as rocks, trunks of trees, banks of sand and mud, &c. must necessarily hinder proportionably the free running off of the water; for it is evident, from what we have said, that the waters so far back from these obstacles, until the horizontal level of the bottom of the bed becomes higher than the top of the obstacles, must be intirely kept up and hindered from running off in proportion

portion thereto (N^o 23.). Now as the waters must continue to come down from their sources, if their free running off is hindered by any obstacles whatever, their relative height back from them must necessarily be increased until their elevation, combined with the velocity of their current proceeding from it, be arrived to such a pitch at the point where the obstacles exist, as to counterbalance the quantity of opposition or impediment proceeding from thence, which frequently does not happen until all the lower parts of the country round about are laid under water.

34. Now it is certain from all experience, that the beds of rivers and canals in general are subject to some or others of the obstacles above mentioned. If rocks or trees do not bar their channels, at least the quantity of sand, earth, and mud, which their streams never fail to bring down, particularly in floods, and which are unequally deposited according to the various windings and degrees of swiftness in the current, must unavoidably, in course of time, fill up, in part, different places in the channel, and thereby hinder the free running off of the back waters. This is certainly the case, more or less, in all rivers, and in all canals of long standing, as is notorious to all those well acquainted with them. Hence, if these accidents are not carefully, and with a constant

constant attention prevented, come inundations, which sometimes lay waste whole districts, and ruin the finest tracts of ground, by covering them with sand: hence rivers become unnavigable, and canals useless, for the purposes for which they were constructed. Canals, in particular, by reason that their waters for the most part remain stagnant in them, are still more liable than rivers to have their beds fill up by the subsiding of mud, and that especially for some distance above each of their sluices; inasmuch, that if continual care is not taken to prevent it, or remedy it as often as it happens, they will soon become incapable of receiving and passing the same vessels as formerly. Nay, the very sluices themselves, if the floors of their bottoms are not of a depth conformable to the bed of the canal, will produce the same accidents as those we have been speaking of; for if they are placed too low, they will be continually filling up with sand or mud; if too high, they have the same effect as banks or bars in the bed of a river, that is, they hinder all the back-waters under their level from running off, and soon fill up the bed to that height by the subsiding of mud. This effect is much accelerated by the shutting of the lower sluices, which makes a great volume of water reflow back to those next above them, till the whole is filled and becomes stagnant. Now it is evident, that

that this state of things must contribute far more to the subsidency of mud and all other matters brought down by the waters in canals, than can be the case in rivers whose currents constantly flow.

35. I do not suppose that these inconveniencies can have yet manifested themselves by any very sensible effects in the many new canals and sluices lately constructed in England; but as the same causes do not cease to act more or less every where, the effects which necessarily follow from them will likewise become more and more sensible, unless continual care be taken to prevent them. The waters of all rivers and canals are from time to time muddy: their streams, particularly during rains and floods, carry along with them earth and other substances which subside in those places where their currents are the least, whereby their beds are continually raised: so that the successive increase of inundations in rivers, and of unfitness for navigation in canals, when they are neglected and left to themselves, is a natural and necessary consequence of the state of things, which no intelligent person can be at a loss to account for; and yet I have known whole countries remain in this habitual state of negligence to their very great detriment.

36. Having thus shewn the principal accidents which rivers and canals are liable to, with the causes of them, I

shall proceed to point out the most efficacious methods of preventing them, or at least of diminishing their effects. Perhaps it would have been more proper to have deferred doing this till I should have said all I have to say upon the nature of rivers and canals: however, I shall forego the more scientific order of things, for the sake of bringing the means of remedying the accidents and inconveniences which happen, nearer to the causes that produce them, whereby their connexion and efficacy may be better judged of. For this end, I shall here lay down, briefly and in general terms, the methods most proper for the purpose in question. They flow immediately from the principles already laid down in this essay, and do not need many words to make them completely understood.

37. A work of this kind, if it is properly conducted, must be begun at the lower end of the river or canal; that is to say, at that end where their waters are discharged into the sea, or where they fall into some other greater river or canal, from whence their waters are carried off without farther hindrance. If it is a river whose bed, by being filled up with mud, sand, or other obstacles, and by being otherwise become irregular in its course, is thereby often subject to inundations, and incapable of internal navigation, the point, from which

the work must be begun and directed throughout all the rest of the channel, is from the lowest water-mark of spring tides on the shore at the mouth of the river; or even something below it, if it can be done; though this part will soon fill up again by the sand, mud, &c. which the tides cease not to roll in.

If it is a canal whose bed is to be dug anew, or one already made, which is to be cleaned and deepened from the sea shore or some large river back into the country, and where no declivity is to be lost; as is the case in all flat countries: the work must be begun, and the depth of the whole channel directed, from the low water-mark of spring tides, if the mouth is to the sea, or from such a depth in the channel of the river, if the canal falls into one, that there may be such a communication of water from the canal to the river, in all situations of the current, as may let boats freely pass from one to the other. This, of course, must also direct the depth of the floor of the last sluice towards the mouth of the canal, be it to the sea or into a river. If the bottom or floor of a sluice already constructed be too low, it will soon fill up with sand or mud, and thereby hinder the gates from opening, unless it be continually cleaned out; if, on the contrary, this floor be too high, and in a canal whose natural declivity is too little for the free current of the water, as is

generally the case in Holland and Flanders, all depth of the bed of the canal below the horizontal level of the bottom of the sluice will serve to no manner of purpose; either for navigation, or for carrying off the back-waters; but will soon fill up with mud, in spite of all means used to the contrary, except that of digging it continually anew to no manner of purpose; as is evident from the reasons given above (N^o 33. 34.):

38. Setting off from this determinate point, at the mouth of a river, or at the bottom of the last sluice upon a canal, which are to be cleaned and deepened; the work must be carried on, in consequence, uniformly throughout their whole course backwards into the country as far as is found necessary for the purposes intended. This is to be done after the following manner :

1st, One must dig up and carry away all irregularities in the bottom and sides of the bed, such as banks of sand and mud, rocks, stumps or trunks of trees, and whatever else may cause an obstacle to the regular motion of the water, and to the free passage of vessels upon it.

2^{dly}, If the declivity of the bed should be still too little to give a sufficient current to carry off the water as often and as fast as is necessary, the whole bed itself must be regularly deepened, and what is dug out from the bottom must be laid upon the sides, to render it narrower in proportion

proportion to its depth. The reason of this is evident from all that has been said.

3dly, Wherever the banks are too low to contain the stream in all its situations, they must be sufficiently raised; which may be conveniently done with what is dug out from the bed: and the whole being covered with green turf will render these banks firm and solid against the corrosion of the water. It is proper at all times to lay upon the banks what is dug from the bed, by which they are continually strengthened against the force of the current.

4thly, It is often necessary to diminish the windings and sinuosities in the channel as much as possible, by making new cuts whereby its course may approach towards a right line. This is a great resource in flat countries subject to inundations; because thereby all the declivity of a great extent of the river, through its turns and windings, may be thrown into a small space by cutting a new channel in a straight line; as may generally be done without obstacle in such countries as I am speaking of, and hereby the velocity of the current will be very greatly augmented, and the back-waters carried off to a surprizing degree, as is evident from what is said above in N^o 29.

5thly,

5thly, Wherever there is a confluence of rivers or canals, the angle of their junction must be made as acute as possible, or else the worst of consequences will arise from the corrosion of their respective streams; what they carry off from the sides will be thrown into irregular banks in the bottom of the bed. This acute angle of junction may always be procured by taking the direction at some distance from the point of confluence.

6thly, Wherever the sides or banks of a river are liable to a more particular corrosion, either from the confluence of streams, or from irremediable windings and turns in the channel, they must be secured against it as much as possible by *weirs*: for this corrosion not only destroys the banks, and alters by degrees the course of the river, but also fills up the bed, and thereby produces all the bad effects we have spoken of above in N° 33. 34. &c.

7thly, But the principal and greatest attention in digging the beds of rivers and canals must be had to the *quantity and form of their declivity*. This must be done uniformly throughout their whole extent, or so much of it as is necessary for the purposes in hand, according to the principles laid down above (in N° 29 and 30.) Conformable thereto, the depths of their beds, and of the floors of their sluices, at the mouths whereby they discharge

charge their waters, being fixed according to what we have said in N° 37. the depth of the rest of the beds, and the quantity of declivity therein, must be regulated in consequence thereof, so as to increase regularly the quantity of declivity in equal spaces the farther we recede from their mouths, and proceed towards their sources or to the part where the regular current is to take place.

If the depth and volume of water in a river or canal is considerable, it will suffice, in the part next the mouth, to allow one foot perpendicular of declivity through six, eight, or even, according to DESCHALES ^(d), ten thousand feet in horizontal extent; at most it must not be above one in six or seven thousand. From hence the quantity of declivity in equal spaces must slowly and gradually increase as far as the current is to be made fit for navigation; but in such a manner, as that at this upper end there may not be above one foot of perpendicular declivity in four thousand feet of horizontal extent. If it be made greater than that in a regular bed containing a considerable volume of water, the current will be so strong as to be found very unfit for the purposes of navigation, as will appear hereafter, when I come to investigate the quantity of declivity in several rivers, the degree of swiftness of whose currents is well known.

(d) De Fontibus et Fluviiis, prop. 49.

39. I dare boldly affirm, from the certain principles of hydrodynamics laid down in this essay, that if the above mentioned things (N° 37. 38.) were carried into execution in a proper manner; the velocity of currents and the acceleration of motion of the waters in rivers, and in canals when their sluices are open, might be increased to any degree that can be required for opening their beds, and for preventing inundations during great rains or sudden floods: by carrying off more swiftly the great accession of water which then takes place. It would not be difficult, by these means, to increase the velocity of the current to double and triple what it is in rivers and canals, whose beds for a long space of time have been left to themselves. There is not, perhaps, a country on earth but what might be freed from inundations by these means. But it may be objected, that if all I have advised was put in execution, even in the flattest countries, the currents of rivers (for canals shut up with sluices are here out of the question) would become incommodious, if not unfit, for navigation, especially against their streams. This objection would be of weight if it was not evident that the various means which I have pointed out may be executed in whole or in part, to a certain degree, and no farther than necessary for the purposes required. But, as it is certain that a strong and regular

regular current in a river is the best of all means for keeping it open and deep, and for preventing the formation of banks in the bed by the subsidence of mud, &c. which it does not allow time to precipitate; I leave it to be considered, whether it is better to have a free and open navigation something incommoded by the strength of the current, or to have soon no navigation at all, without repeatedly digging the bed anew.

40. I shall not here enter into the mechanical part of the methods of digging and cleaning canals, rivers, and sea ports, or into any description of the machines and instruments necessary for that purpose. The subject would lead me much too far: besides all these things may be found much at length in most of the authors who have wrote upon hydraulic-architecture, such as BARATTERI, CORNELIO MEYERI, GUGLIELMINI, and a notorious anonymous French plagiarist, who has taken from MEYERI, without ever naming him, almost all that is contained in his book, published at Paris in 1693, and at Amsterdam in 1696, in octavo, under the title of *Traité des Moyens de rendre les Rivières navigables*. But the author who has treated this subject with the greatest care, and most at length, is the celebrated BELIDOR, in his *Architeſture Hydraulique*, 4 vol. in quarto. To these may be added a late memoir of M. FORFAIT of

Rouen, vice-architect of the French navy, which gained the prize of the Royal Academy of Sciences and Belles Lettres of Mantua, for having given the best solution of a problem proposed by that Society in 1776, in the following terms: "*To indicate the best and cheapest method of freeing navigable canals from banks of sand and earth formed in their beds which render them too shallow.*" This piece, printed at Mantua, by Pazzoni, in 1778, contains sixty-three pages in quarto, and is divided into two parts; the first contains the means of preventing the formation of banks in navigable canals; and the second offers divers methods for remedying them when they are already formed. For this purpose the author proposes six different machines of his own invention: the first may be employed in rivers near the sea, and subject to the ebb and flow of the tides; the second may be used in those where the waters are always nearly of the same height and velocity; the third and fourth are to be used in those places where the violence of the currents corrode the beds; and the two last serve to break up the banks of sand or earth formed in the bottom, and to carry off all heterogeneous bodies sunk in the river, which cause an obstacle to the current. It would be difficult to give a just idea of these machines without the help of the six plates which accompany the piece;

piece; but as this production of a foreigner has been crowned in Italy, the country of all others in which, from all antiquity, the science of rivers and canals has been most cultivated, we cannot well doubt of its merit, or that it is worthy of a translation into our own language.

V. Other considerations on the nature of rivers and inundations.

41. Rivers flowing along plains, as well as through vallies, have naturally their beds in the lowest part of the ground comprized between the opposite hills or mountains: nevertheless, the surface of the water of a river in the midst of a plain is often higher than the surface of the grounds adjacent to the banks of the river. This proceeds from the continual subsiding of the mud, &c. brought down by the stream during floods; the waters in that case usually overflowing the banks spread themselves over the plain, where they lose a great part of the swiftness of their current, which contributes greatly to the subsiding of the mud they contain; so that the farther they flow upon the plain, the clearer they grow, and the less remains to subside. From hence the greatest precipitation of mud must be in the parts of the plain nearest the sides of the river, which in length of time will raise

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these

these grounds above the rest of the plain. Again, the waters in the bed itself depositing incessantly a part of the mud, &c. brought down by the stream, must continually, though insensibly (for a long space of time) raise the channel and banks of the river above the rest of the plain. These causes may at last contribute to the forming of an entire new bed for the river; for as all rivers carry down in their streams more or less mud and other heterogeneous matters, which do not subside regularly in all parts alike, but must precipitate fastest where the current is slowest; there must accumulate by little and little in these parts such banks of sand and mud, as will in time hinder the current of the waters, make them re-flow, and at last totally change their direction.

Canals are still more subject than rivers to have their beds raised and their currents stopped by the subsiding of mud and heterogeneous matter in different places, and especially just above their sluices; because of the sudden stagnation of the water which first begins there as often as the sluices are shut: and as there is a necessity for keeping them for the most part shut, the stagnating waters in their beds must precipitate their mud, &c. in a much greater proportion than can be done in the currents of rivers, which are in a continual motion towards the sea.

42. I call *center of the current*, or, more properly, *line of greatest current*, that *line which passes through all the sections of a river, in the point where the velocity of the current is the greatest of all*. We have seen above (N° 25.) that if the current of a river is regular, and in a right line, its center or line of greatest velocity will be precisely in the center of the sections (N° 6.): but, on the contrary, if the bed is irregular and full of turns and windings, the center or line of greatest current will likewise be irregular, and often change its distance and direction with regard to the centers of the sections through which the waters flow, approaching successively, and more or less, to all parts of the bed, but always in proportion and conformably to the irregularities in the bed itself.

This deviation of *the line of greatest current* from the centers of the sections through which it passes, is a cause of many and great changes in the beds of rivers, such as the following:

If, In a straight and regular bed, the greatest corrosion of the current will be in the middle of the bottom of the bed; because it is that part which is nearest to the line of greatest current, and at the same time which is most acted upon by the perpendicular compression of the water. In this case, whatever matters are carried off from the bottom will be thrown, by the force of the current,

current, equally towards the two sides, where the velocity of the stream is the least in the whole section.

2dly, If the bed is irregular and winding, the line of greatest current will be thrown towards one side of the river, where its greatest force will be exerted in proportion to the local causes which turn it aside: in short turns of a river there will be a gyration, or turning round of the stream, by reason of its beating against the outer side of the angle; this part will be corroded away, and the bottom near it excavated to a great depth. The matters, so carried off, will be thrown against the opposite bank of the river where the current is the least, and produce a new ground, called an *alluvion*.

3dly, Inequalities at the bottom of a river retain and diminish the velocity of the water, and sometimes may be so great as to make them reflow: all these effects contribute to the subsiding of sand, earth, and other matters thereon, which cease not to augment the volume of the obstacles themselves, and produce shallows and banks in the channel. These in time, and by a continuance of the causes, may become islands, and so produce great and permanent changes and irregularities in the beds of rivers.

4thly, *The percussions of the center of the current against the side of the bed are so much the greater as they are made*

made under a greater angle of incidence; from whence it follows, that the force of percussion, and the quantity of corrosion and of detriment done to the banks and weirs of rivers, and to the walls of buildings made therein, and which are exposed to that percussion, *are always in a direct compound proportion of the angle of incidence, of the greatness and depth of the section together, and of the quantity of velocity of the current.*

5thly, It may happen in time, that the excavation of the bottom, and the corrosion of the sides, will have so changed the form of the bed as to bring the force of percussion into equilibrium with the velocity and direction of the current; in that case, all farther corrosion and excavation of the bed ceases (N^o 31.)

6thly, This gives the reason why when one river falls into another almost in a perpendicular direction, and makes with it too great an angle of incidence, this direction is changed in time, by corrosions and alluvions, into an angle much more acute, till the whole comes into equilibrium:

7thly, So great and such continued irregularities, from local causes, may happen in the motion of a river, as will intirely change its ancient bed, corrode through the banks, where they are exposed to the greatest violence of percussion.

percussion of the stream, and open new beds in grounds lower than what the old one is become.

8thly, Hereupon the state of the old bed will entirely depend on the quantity of water, and on the velocity and direction of the current in the new one; for immediately after this division of the waters into two beds is made, the velocity of the current in the old one will be diminished in proportion to its less depth. In consequence thereof, the waters therein will precipitate more of their mud, &c. in equal spaces than they did before; which will more and more raise up the bottom, sometimes even till it becomes equal with the surface of the stream. In this case, all the water of the river will pass into the new bed, and the old one will remain entirely dry. It is well known, that this has happened to the Rhine near Leyden, and to many other rivers.

9thly, Hence the cause of the formation of the new branches and mouth, whereby many great rivers discharge their waters into the sea.

43. But in proportion as *a river, that has none of these obstacles in its bed*, approaches towards its mouth, we see the velocity of its current augment, at the same time that the declivity of the bed diminishes, the causes of which have been explained above (N^o 29.). It is for this reason, that inundations are more frequent and confi-

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derable

derable, and do more damage in the interior parts of a country, than towards the mouths of most rivers.

In the Po, for example, the height of the banks made to keep in the waters diminishes as the river approaches to the sea. At Ferrara they are twenty feet high; whereas nearer the sea they do not exceed ten or twelve feet, although the channel of the river is not larger in one place than in the other.

44. The mouths of rivers, by which they discharge their waters into the sea, are liable to great variations, which produce many changes in them.

1st, The velocity and direction of the current at these mouths are in a continual variation, caused by the tides, which alternately retard and accelerate the stream.

2dly, During the flowing of the tide, the current of the river is first stopped, then turned into a direction intirely contrary throughout a considerable extent; if we may believe M. DE BUFFON, there are rivers in which the effect of the tides is sensible at 150 or 200 leagues from the sea.

3dly, This state of things is a cause of a great quantity of sand, mud, &c. being precipitated and accumulated in the channel near the mouth. This continually raises and widens the bed, and at last changes it intirely into a new place, or at least opens new mouths to dis-

charge the waters at. The Rhine, the Danube, the Wolga, the Indus, the Ganges, the Nile, the Mississippi, and many other rivers, are instances of this.

4thly, All these effects are less sensible at the mouths of little rivers, as their currents oppose no sensible obstacle to the flowing of the tides; so that the ebb carries off again what the flow had brought in.

45. Whenever the course of a river throughout a considerable extent of country approaches towards a right line, its current will have a very great rapidity; and the velocity wherewith it runs diminishing the effect of its natural gravitation, the middle of the current will rise up, and the surface of the river will form a convex curve of sufficient elevation to be perceived by the eye; the highest point of this curve is always directly above *the line of greatest current* in the stream.

On the contrary, when rivers approach near enough to their mouths for a sensible effect to be produced in them by the flowing of the tides; and also when in other parts of their course they meet with obstacles at the sides of their channel: in both these cases the surface of the water at the sides of the current is higher than in the middle, even though the stream be rapid. In this situation of things, the surface of the river forms a concave curve, the lowest point of which, or that of inflexion, is

directly over *the line of greatest current*. The reason thereof is, that there are in this case two different and opposite currents in the river; that whereby the waters flow towards the sea, and preserve their motion therein even to a considerable distance; and that of the waters which remount, either by the flowing of the tide, or by their meeting with local obstacles, which form a *counter current*, so much the more sensible as the flowing of the tide is stronger, or as the percussive of the water is made against greater obstacles, and in a direction nearer to a perpendicular to them. From both these causes, the greater of which by far is that of the tides, the water near the sides of the channel, where the velocity of the descending stream is naturally the least (N° 25), takes a contrary direction, and runs back in the river, while that in the middle continues to flow on towards the sea. This counter current is what the French call *a remous*.

An island in the middle of a river produces the same effect as obstacles at the sides, regard being had to the difference of situation of each.

Eddies and *whirlpools* in rivers, in the center of which there appears a conical or spiral cavity, and about which the water turns with great rapidity and sucks in whatever approaches it, proceed in general from the mutual percussive of these two counter currents; and the vacuity in

the middle is produced by the action of the centrifugal force, whereby the water endeavours to recede, in a direct ratio of its velocity, from the center about which it moves.

46. If rivers persevered always nearly in the same state (N^o 5.) the best means of diminishing the velocity of the current, when it is found too great for the purposes of navigation, would be by widening the canal: but as all rivers are subject to frequent increase and diminution, and consequently to very different degrees of velocity and force in the current, this method is liable to produce very detrimental effects; for, when the waters are low, if the channel is very large in proportion, the stream will excavate a particular bed, which, according to the irregularities of the bottom, will form various turnings and windings with regard to the principal bed; and when the waters come to increase, they will follow, to a certain degree, the directions which the bottom waters take in this particular bed, and thereby will strike against the sides of the channel, so as to destroy the banks and cause great damages.

It would be possible to prevent in part the bad effects proceeding from the current striking against the banks, by opening, at those places where it strikes, little gulfs into the land, dug in such a form and direction as that the striking

striking current should enter and circulate therein, so as to destroy, or at least greatly diminish, its velocity. This effect would be felt for a considerable way down the river.

This same method might probably be used with success against the destruction of bridges, weirs, &c. by the violence of the stream during floods. Such gulfs being dug into the outer side of those turnings in the river which are immediately above the place to be secured from the violence of the stream, would successively diminish its velocity, its force and dangerous effects, a considerable way down the river. It is true, this method might contribute to produce an overflowing of the river upon the grounds adjacent to those artificial gulfs, this being a natural consequence of the decrease of the velocity of the current in those places; and it would remain to be considered whether those local inundations, or the danger of destruction of the bridges or edifices in the river, were the lesser evil.

47. The nature of inundations, and the manner of their formation, merit a particular attention in this place.

While the volume of water in the *bed* of a river increases, the velocity of the current increases in proportion, as has been repeatedly shewn above (N° 13. 18. 20.

23. 27. 28. 29.). But from the moment that part of this water overflows the bed, the velocity thereof begins to diminish (N° 41.) and does so more and more, the farther it flows and spreads on the plain. So that the overflowing being once begun, it is a natural consequence, that the inundation should continue for several days; for though the volume of water brought down by the flood during that time should decrease, yet, as the quantity of what runs off decreases likewise, from the great decrease of velocity in what overflows the plains, it will continue to produce the same effect as if the volume of water coming down had not diminished, until the whole of the stream be every where contained again within the bed of the river. When that is become the case, the waters that have overflowed the plain will decrease thereon, by gradually and slowly running off, and also by evaporation, till they wholly disappear. If this was not so, we should see rivers overflow for an hour or two, and then return again within their beds, a thing contrary to general observation; for we constantly see inundations, once begun in flat countries, last for several days together, although in the mean while the rain ceases, and the quantity of water coming down diminishes. This must be the case, because as the overflowing diminishes the velocity, and consequently the quantity of water carried

carried off, it has the same effect as if a greater quantity still continued to come down.

It may not be useless to remark here, that what we have often said in this essay becomes evident from these observations on nature, as well as from the principles laid down in it; to wit, that the most direct and efficacious method of preventing inundations is by deepening the bed and raising the banks of the river.

It may likewise be observed, with regard to inundations, that if the wind blows directly contrary to the current of the river, the overflowing will be greater than it would have been otherwise, because this accident diminishes the velocity of the stream: but, on the contrary, if the winds blow in the same direction with the current of the river, the inundation will be less than otherwise, and sooner at an end; because this accidental cause augments the velocity of the stream.

VI. *On the confluence of rivers, and on the separation of the same river into divers branches and mouths, with the effects thereof upon the velocity of currents, inundations, &c.*

48. All great and long rivers receive into their beds many others of different magnitude throughout the extent

tent of their course. This is evident to every one who only casts his eyes over a map. The Rhine and the Po, in particular, receive each above one hundred others great and small; the Danube above two hundred; the Wolga as many; the river of Amazons receives into its vast bed a prodigious number, some of which are five or six hundred leagues in length, and are of such a depth and breadth as would make them elsewhere pass for capital rivers. M. DE BUFFON ^(c) gives a list of the more considerable of those which fall into other great rivers throughout the known world. Many curious particulars may be seen in VARENIUS'S General Geography, part I. chap. XVI. concerning rivers; but of a nature which does not enter into my plan. The works themselves are in every body's hands, and may be consulted by those who please.

This confluence of rivers is so necessary for propagating the motion of the water throughout a long course, and for renewing and accelerating from time to time its velocity, which otherwise would be too greatly diminished by the resistance of so many obstacles as they meet with in their way, that, as we have said above (N° 28.) after Signor GUGLIELMINI, it can only be attributed

(c) Hist. Nat. tom. II. p. 75, 76.

to the infinite wisdom of the Author of Nature, in the original disposition of things.

49. We have seen above (N^o 18. 27. 28.) that the increase of a river or canal by the new waters which it receives, is *universally in an inverse ratio of the new velocity which is acquired therefrom*. If this velocity is greater, the increase of the section of the new stream will be less in proportion, and *vice versa*. It follows from hence, that it is possible for one river or open canal to fall into another river or open canal of equal magnitude with itself, and yet the section of the current in the common bed after their confluence shall be no greater than it was in each of them before their junction. It is certain that this will be the case as often as *the confluence of the two augments the velocity of the common current in the same proportion with the increase of the volume of waters*; both the greater rapidity of the current, and the greater volume of water in the bed after the junction, serving to deepen it in proportion to its breadth, will contribute towards the above effect. Another cause will likewise add thereto; to wit, that instead of the resistance from the attraction, friction, and other obstacles, in two beds, which give two bottoms and four sides, there are, after the confluence, only those of one bed, of one bottom and two sides. Moreover, the center of the section in the common bed

is farther from the bottom and sides thereof, than it is in the separate beds. All these causes, in proportion to their respective quantities, contribute to accelerate the velocity of the common stream.

50. It is not less certain, that in rivers which bring down a great abundance of water, the more the velocity and discharge thereof at their mouths are retarded and diminished by the tides, the winds, the rolling in of the sea, &c. the more will the back-waters increase in height, and endanger overflowing the inner parts of the country. *This is evident, because the decrease of velocity in the current, and the increase of height of all the back-waters that are affected thereby, are in a reciprocal inverse ratio one of another (see above N° 32.).

Nature itself teaches us a method of preventing, or at least of diminishing, this effect. We see all great rivers overcharged with a vast volume of water divide, when they come near the sea, into different branches and mouths, whereby the super-abundance of their waters is discharged. This is the case with the Scheldt, the Rhine, the Rhone, the Po, the Danube, the Wolga, the Euphrates, the Indus, the Ganges, the Nile, the Niger, the Oroonoko, the River of Amazons, and with almost all other great rivers.

This separation and dispersion of the too great quantity of water into several channels is one cause of their seldom overflowing the country near their mouths ^(f), because it gives a greater depression and declivity to the surface of the current, and thereby facilitates the running down of the waters from the interior parts of the country, forasmuch as their beds are every where regular and free from obstacles to their current.

§ 1. Notwithstanding the apparent opposition to what has been said in several other parts of this treatise, I repeat again, that this division and dispersion of the waters into several branches and channels *when there is such an abundance of it as is sufficient to keep up the velocity both in the old and new channels*, augments the declivity, and thereby facilitates the running off of all the back-waters from the inner parts of the country, as far as the bed is regular and free from obstacles, according to what is laid down above (N° 38.).

But whenever this super-abundance of waters, sufficient for keeping up the velocity in each channel nearly to what it was before the separation or divarication, shall be found wanting, it is certain, that this division and dispersion of the waters into several channels will only serve to diminish the velocity of the current in each, whereby

(f) Another cause thereof is pointed out above, N° 43.

as much or more discharge of the water, and consequently of declivity for the running off of the back-waters, may be lost, as has been gained by the separation into different beds.

This disadvantage may be easily remedied in those new channels and mouths of rivers which are dug by hands, and have sluices placed in them at the point of separation from the original bed; for these sluices of communication need be opened only when there is a super-abundance of water in the river, sufficient to keep up the velocity in each of the channels; at other times they may be kept shut, and the waters retained in their original bed.

52. It was for this purpose, of preventing the damages proceeding from immoderate inundations, that the ancient Egyptians dug vast lakes, and made so many canals and sluices of communication between the Nile and those lakes, and from thence to the sea; that they might thereby be able to discharge the waters into those reservoirs if they came down in too great abundance, or let them off again from thence upon the land, if the quantity of the natural inundation at any time was less than what was necessary for the good of the country. By these means ancient Egypt was always master of its waters.



It is well known that it rains seldom in that country, and that the Nile by its regular inundations waters the land, by bringing down upon it the rains and melted snow from the high mountains of Abyffinia. HERODOTUS ^(g) and DIODORUS SICULUS ^(h) have left us descriptions of the immense labours of the inhabitants to govern and multiply so beneficent a river, the particulars whereof are too well known to be repeated in this place. By these means Egypt became the granary of the world for above two thousand years, and reimbursed, with immense advantage, the first expences.

RICCIOLI ⁽ⁱ⁾ assures us, that the ancient Persians did the same thing with regard to the *Euphrates*, and for the same end. He adds, moreover, *Sic ubi Cyrus Gangem in Alveos 460 disperfit, minora damna ex Gangis alluvionibus campi perpeffi sunt*; but I am totally at a loss to find upon what authority he grounds this last assertion, for I never read that any CYRUS penetrated as far into India as the mouth of the *Ganges*, much less reigned so long over that country as to perform the vast work which RICCIOLI speaks of.

(g) In lib. II.

(h) Biblioth. l. II. c. 1.

(i) Geogr. et Hydrogr. l. VI. cap. xxix. p. 248, 249.

PLINY ^(k) says, with regard to the different mouths of the Po, *Omnia ea flumina fossasque primi à Sagi fecere Tbusci, egesto amnis impetu per transversum in Atrianorum Paludes, quæ Septem Maria appellantur.* These seven lakes discharged their waters into the sea by seven mouths, which PLINY names in the same place. All this was apparently done that the river might do less damage to the adjacent countries by its frequent inundations. PLINY adds, *His se Padus miscet, ac per bæc effunditur, plerisque, ut in Ægypto Nilus, quod vocant Delta.*

To these examples, drawn from ancient history, might be added many modern ones, if the things in question had need of further proofs. Thus, both nature and the experience of a long series of ages teach us, that the separation of a river into several beds, by new branches and mouths, is a means of diminishing inundations in the inner part of the country; but that this takes place only when there is a sufficient abundance of water in the river to fill the new beds and channels so far as to prevent the velocity of the currents therein from being notably diminished from what they were before the division.

(k) Hist. Nat. l. III. cap. xvi.

S E C T I O N II.

Laws of the meeting of opposite currents, with the application of them to sluices.

53. When two equal currents of homogeneous fluids meet in opposite directions, there is first a swelling and rising up of them at the point of rencounter; then follows a revulsion and counter current of each equally back again, so as to bring the whole to an equilibrium.

54. If the two opposite currents are unequal, either in force or in quantity, or in both, there will still be a swelling and revulsion of each back again, but it will be diminished in the greater current, and augmented in the lesser, by the quantity by the which the one surpasses the other; and the point of rencounter of the two currents will have a slow and progressive motion in the direction of the stronger, the degree of velocity thereof being always in a direct ratio of the force and quantity of the one above the other.

55. If the fluids in opposite currents be not homogeneous, as is the case between sea and river water, that which has the least specific gravity will swim upon the other, and continue to follow its first direction, until
such

such time as the heavier fluid shall have communicated its motion to all the parts of the lighter. But the lighter fluid will not lose its former motion and direction at once, but *in a decreasing series, the law whereof will vary according to the greater or less difference of specific gravity in the two fluids*, until the whole of the lighter has acquired the velocity and direction of the heavier which buoys it up.

The *time* and *space* required for a greater current of salt water to communicate its motion and direction to an opposite one of fresh water will be but very little, since they differ in specific gravity only $\frac{3}{73}$ parts that the salt is heavier than the fresh. It would require much greater between water and oil, and still much more between quicksilver and oil, and so on. The elements for determining them in every case might be found by a proper number of experiments.

56. Let the two currents be equal or unequal in force and velocity *but nearly of the same specific gravity*, if we should suppose at the same time that their surfaces are not upon a level, but that the one is higher than the other (as is constantly the case in all sluices that open to the sea, except at the moment when the surface of the tide is upon a level with the surface of the water in the canal behind the sluice); this circumstance entirely changes both the case and the effects. It is certain, on
this

this supposition, that the overplus of velocity and elevation in the higher current, though it should be the lesser, will make the waters in the lower and greater current reflow upon themselves until they come to a level and equilibrium with those in the upper current; since these, by the laws of universal gravitation, cannot flow back from a lower to a higher level, but must descend according to the declivity of the surfaces. If the currents are of *very different specific gravities*, they will come to an equilibrium according to the law laid down above (N° 55); but their greater or lesser quantity and velocity will produce little or no effect in this case.

57. Now as the running of two currents in opposite directions, after their rencounter, and beyond the limits laid down above (N° 55.), is incompatible with, and contradictory to, the laws of nature, and consequently impossible; we may draw this useful conclusion, which becomes important during inundations, and especially during the annual overflowing of the low grounds in flat countries; to wit, that if the sluices next the sea against which the tide flows be shut only a quarter of an hour before the *flood* has risen to the level of the water in the canal, not a drop of salt water can enter the said canal, nor even into the sluice itself; because both the progressive motion of *the point of rencounter of*

the two currents, and the over-swimming of the fresh water upon the salt, will be always without the sluice and towards the sea, so long as the surface of the tide is below the level of the water in the canal. Many sluice masters, for want of knowing or considering this, are accustomed to shut their gates next the sea a little after half flood, under the pretence of preventing by this means the salt water from getting into the canal, and communicating thereby with the waters that overflow the low grounds in many places during winter, which would be of great detriment to the soil. Through this false persuasion, they lose no inconsiderable part of that time every day, which they might safely employ in drawing off the waters which overflow and incommode low and flat countries almost every winter and rainy season, as is the case in the Dutch and Austrian Netherlands.

S E C T I O N IV.

Experiments to determine the different velocities, in different depths of water, of the same floating body moved uniformly by an equal force.

58. It is well known already, that for facilitating or retarding the motion of boats, &c. in canals, the different
I
depths

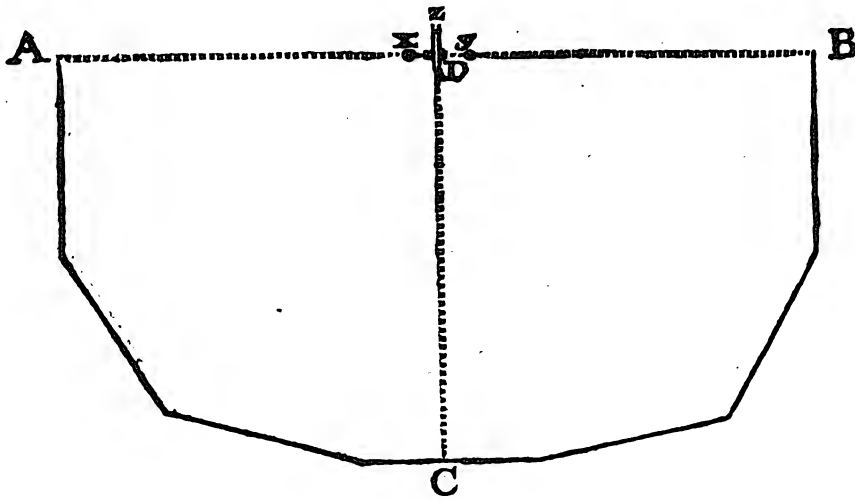
depths of the water, above that simply necessary to keep them afloat, is a thing not at all indifferent. Dr. FRANKLIN has already treated this subject, though perhaps not with sufficient accuracy, in a letter to Sir JOHN PRINGLE, written in the year 1769. He proves, however, that it is universally known among people accustomed to work boats on canals, that there is a considerable difference in the swiftness of their motion according to the greater or less depth of the water therein; and that the water being low is of itself sufficient to retard the motion of a boat, without the keel thereof rubbing against the bottom of the canal. The reason he assigns for it is evident; for a boat cannot advance its own length in a canal without displacing a quantity of water equal in mass to the space which the boat occupies under the surface of the fluid. The water so displaced must retrograde, and pass under, and to the right and left, of the boat: so that the less depth and breadth of water there is in the channel, the more in proportion it must rise up and weigh against the boat, and the more difficulty it must find in passing under and along side of it, and necessarily must retard so much the more the motion thereof. The result of Dr. FRANKLIN's experiments on this subject may be seen in the letter above mentioned.

59. Mr. NEEDHAM, Director of the Imperial Academy of Sciences at Bruxelles, being of opinion that Dr. FRANKLIN'S experiments were made upon too small a scale to draw any very exact inferences from them, desired me, at the beginning of the year 1775, to make a new set of experiments upon a much larger scale and with all possible exactness; I did accordingly, and shall here give a short description of them.

I got made, by the ship carpenters of Nieuport upon the Coast of Flanders, an exact model of a bilander, answerable in all its proportions to those used in the Low Countries. Its length was thirty-nine English inches, its breadth nine inches and a half, and its depth nine inches. Its form both within and without exactly represented that of a bilander. At each end of it was fastened perpendicularly a round and polished rod, ten inches and a half in height above the sides of the boat.

I got made likewise a wooden canal, twenty feet in length, thirty-seven inches in breadth, and sixteen inches in depth; a section of which is represented in the following figure:

This



This form is that of the excavation of the canals in the Low Countries, and approaches to that of the natural beds of rivers inasmuch as they are regular. Here $AB=37$ English inches, $DC=16$ inches; and the length of the whole canal, as we said before, was twenty feet. z represents the section of a pulley fixed at one end of the canal, upon which passed a small cord, one end of which was tied to the round rod at the fore part of the boat, and at the other end was a piece of lead which weighed eight ounces. This served for an equable force to give an uniform motion to the boat throughout all the experiments. x and y are sections of two other cords stretched parallel to each other at about one inch and a half distance, and reaching from one end of the canal to

the other. The two round rods fixed at the ends of the boat, moving within these parallel cords, served to make the boat move in a right line in the middle of the canal, without running against either side, which it would have done without this precaution. The canal itself was upon an exact level, and one end of it, where the pulley was fixed, rested upon the side of a well twenty-three feet deep, twenty of which were above the surface of the water; which gave sufficient space for the free and uniform descent of the lead-weight and cord running over the pulley, as they drew the boat from one end of the canal to the other.

Latts, exactly divided into inches, were nailed against each end of the canal within, to mark the different depths of the water in it according as it should be augmented or diminished. The outsides of the little boat, from its keel upwards, were likewise divided into inches. In the inside of the boat was a quantity of sand sufficient to sink it to six inches deep in the water. The common loaded bilanders in the Low Countries usually draw six feet of water.

Thus the form of the wooden canal, together with its breadth and depth, and the form and dimensions of the little boat, together with the depth of water it drew by means of its ballast of sand, exactly corresponded with those

those in the real canals and bilanders in the Low Countries, an inch in the one answering to a foot in the other.

Close to the canal, and out of the way of all wind, was suspended a pendulum of fine waxed thread, to prevent the variations of the atmosphere from altering its length, which from the point of suspension to the center of gravity in the lead was $39\frac{1}{5}$ English inches, so that its isochronic vibrations were exactly seconds of time.

60. It was necessary, in order to render the experiments exact, that they should be made at a time when the air was perfectly calm; for the least breath of wind, during the motion of the boat, caused great variations and irregularities in them, which it was absolutely necessary to prevent, in order to be able to deduce any exact results from them. On the contrary, in a perfect calm, the times of the passage of the boat, from one end of the canal to the other, were exceedingly regular, as may be seen from the table of experiments which I give below.

By means of the pendulum I was able to measure the times of passage of the boat along the canal, in all the different depths of water, to a third or even to one quarter of a second. The boat being held fast against the back end of the canal by the hand of an assistant, and then let go, it was easy for me to perceive the precise instant

instant of the beginning of its motion, to let go the pendulum at the same moment, and to count its vibrations till the instant that the boat struck with an accelerated force against the fore end of the canal. As to the weight of eight ounces suspended at the end of the cord, and which served as *a moving force* to draw the boat along the canal, it was just as much as sufficed to counter-balance the cord, and to put the boat in motion; less weight than that would do neither: therefore I was obliged to use so much, notwithstanding the considerably accelerated motion it gave to the boat.

This is the whole mechanism of the instruments I used for the experiments in question, and such were the precautions I judged it necessary to use for making them with scrupulous exactness.

In the following table, which consists of twelve columns, the first of them contains the different depths of water at which the experiments were made; the ten following ones contain ten different experiments made at each depth of water in the canal; and the twelfth or last column is the reduction of the ten others to a mean proportional or mean result of the whole, which is in *seconds of time*.

Table

Table of experiments made to ascertain the times of passage of the boat along the canal, or its different degrees of velocity, in different depths of water.

Depths of Water.	1st Exp.	2d ditto	3d ditto	4th ditto	5th ditto	6th ditto	7th ditto	8th ditto	9th ditto	10th ditto	Mean Results.
Inches											
15	14 $\frac{1}{2}$	14 $\frac{3}{4}$	14 $\frac{1}{2}$	14 $\frac{1}{2}$	14 $\frac{1}{2}$	14 $\frac{3}{4}$	14 $\frac{3}{4}$	15	14 $\frac{1}{2}$	14 $\frac{1}{2}$	14 $\frac{1}{2}$
14	14 $\frac{3}{4}$	15	15	15 $\frac{1}{2}$	15	15	14 $\frac{1}{2}$	14 $\frac{1}{2}$	15	15 $\frac{1}{2}$	15
13	15 $\frac{1}{2}$	15 $\frac{1}{2}$	15 $\frac{1}{2}$	15 $\frac{3}{4}$	15 $\frac{1}{2}$	15 $\frac{3}{4}$	15 $\frac{1}{2}$	15 $\frac{1}{2}$	15 $\frac{1}{2}$	15 $\frac{1}{2}$	15 $\frac{1}{2}$
12	16 $\frac{1}{2}$	16	16	15 $\frac{1}{2}$	16	15 $\frac{3}{4}$	16	16	16	16	16
11	16 $\frac{1}{2}$	17	17	16 $\frac{1}{2}$	16 $\frac{1}{2}$	17	16 $\frac{1}{2}$	16 $\frac{1}{2}$	16 $\frac{1}{2}$	16 $\frac{1}{2}$	16 $\frac{1}{2}$
10	17	18 wind	17	17 $\frac{1}{2}$	17	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17	17	17 $\frac{1}{2}$	17 $\frac{1}{2}$
9	18 $\frac{1}{2}$	18 $\frac{1}{2}$	18	19	19	18	18	18 $\frac{3}{4}$	18	18 $\frac{1}{2}$	18 $\frac{1}{2}$
8	20	20	20 $\frac{1}{2}$	20	20	20	20	19 $\frac{1}{2}$	20	20	20
7	23 $\frac{1}{2}$	23	22 $\frac{1}{2}$	23	23	23	23 $\frac{1}{2}$	23 $\frac{1}{2}$	23	23	23
6 $\frac{1}{2}$	In the experiments made with this depth of water, the boat often touched the bottom.										30 by supposition

It may be observed with regard to the last column of the above table, which contains the mean results or average times which the boat takes to pass

shews, as nearly as possible, what the true time of passage ought regularly to be when nothing happens to disturb it.

It is also highly worthy of remark, that the *mean results* contained in this last column form a *series of numbers regularly increasing as the depths of water, wherein the respective experiments were made, regularly decrease*; so that *the different velocities of the floating body are in an inverse ratio of the respective depths of the water in which it floats with an equal impulsive force, and that according to the law of the above series.* This, perhaps, may furnish elements to calculate, pretty near the truth, the different velocities of vessels upon canals and rivers with different depths of water in all other cases whatsoever. As to the conclusions to be drawn therefrom in practice, and in the common uses of life, they are too obvious to need mentioning here.

S E C T I O N V.

On the quantity of declivity in rivers.

62. Abstracting from all resistance and friction, fluids, such as water, descend upon planes let them be never so little inclined towards the center of the earth: and the
 5 velocity

velocity of descent increases in a compound ratio of the increase of the mass of water, and of the greater declivity of the plane which serves for its bed (N° 13.).

63. Water, though unaffected by any compression or impulsion from above, cannot remain immoveable in any bed whatever except that which makes a curve perfectly concentric with the *terrestrial curve*; but in this, being every where equally affected by the force of gravitation, it will remain without motion any way.

64. It follows from hence, that the springs and sources of all rivers must be at a greater distance from the center of the earth than one semi-diameter thereof, which is terminated at the surface of the sea; without which the waters could not run to the river's mouth.

65. Therefore, the absolute elevation of the surface of rivers is continually diminished as they recede from their springs, because of the necessary declivity of the beds of rivers towards the center of the earth; for without some degree of this declivity the waters could not run at all, as has been said above (N° 62. 63.).

66. The declivity of the beds of rivers cannot be a right line making a rectilinear angle with that horizontal plane which, being continued, would intersect their respective sources; but, if it is regular, it must be a curve which differs very little from that of the earth's surface,

and this, if the direction is in the parallels of latitude due East and West, is *spherical*; but in all other directions it is a portion of an *oblate ellipsis*, on account of the earth's being a spheroid compressed by its axis. Now the horizontal plane which continued passes through the springs of rivers, is always a tangent to the curves of their beds at the point of inflexion, inasmuch as these are regular.

67. The quantity of absolute declivity from the spring in any determinate part of a river, is that perpendicular line drawn from the point of greatest current in that place till it meets *the curve concentric to the earth's surface* which passes through the river's source. The declivity of the bed below the spring is had by taking the same perpendicular from the bottom of the bed; as that of the river's surface is by taking the perpendicular from thence.

68. If a plane be extended horizontally every way from the point of tangency to the earth's surface, or from the point where it is perpendicular to any radius of the earth, water will run from every other part of the plane towards that said point which is nearer to the center of the earth than any other point in the whole plane.

69. The depression of *the curve of a river's bed*, below *the concentric-terrestrial-curve which intersects its source,*

source, being only 250 fathom perpendicular in a course of 500 leagues, it will be sufficient to give a notable current in a regular bed throughout all that extent of river, as appears from what we have said above (N° 38.). But the depression of *the curve of the river's bed*, below *the horizontal plane which is a tangent to its source*, in this same extent of course, is not less than ninety leagues perpendicular, being always *the secant of the arc of the river's extent minus a radius of the earth in that point*.

70. It follows evidently from the above principles (N° 62—69), that the declivity and velocity of a river are less in proportion as the bed approaches nearer to being concentric with the curve of the earth's surface.

71. I shall now apply the principles laid down to determine, as near as possible, the real quantity of declivity in different rivers, making use of what is already known from experiments and actual mensuration to determine the same in all others by the comparison of the different degrees of velocity in their respective currents.

72. It is the general opinion of most of those who have examined this subject^(k), that rivers and canals which have less than one foot of declivity in 10,000 feet of course, will have very little current, unless it be by means of the great abundance of their upper waters

(k) Vide DESCHALES, de Font. et Fluv.

which give motion to those before them by their weight and impulsion. Without this the resistance proceeding from the bottom and sides of the bed, and from other accidental obstacles (N° 21.) would equal, if not surpass, the ordinary causes of acceleration (N° 20) so as to diminish continually the motion of the waters, and at last render them almost stagnant (N° 22.). But nature has prepared remedies against this, as we have seen above (N° 28. 29. 48.). What RICCIOLI ⁽¹⁾ says of the *Po*, in that part of its course next its mouth, is perfectly conformable to this theory: “ Sic Padus, qui a Pago *Ostellata* “ vocato, usque ad *Adriaticum*, intervallo milliarium cir- “ citer 70, non habet libramentum majus 13 aut 14 “ pedum, ita ut singulis miliaribus ne 3 quidem unciae “ declivitatis obveniant; unde *Padusa*, potius instar stag- “ nantibus aquis, incertissimus esset ad defluxum cursus: “ impetu tamen impresso à 30 et amplius fluminibus “ aut torrentibus se in illum exonerantibus, etiamque à “ nativæ pondere aquæ ex superioribus et altioribus “ prope Alpes alveis decurrentis, velocitatem maximam “ acquirit.”

73. From many observations and trials which I made for this purpose in the years 1773 and 1774 upon the river Iprelee in Flanders, which comes down from the

(1) Geogr. et Hydrogr. l. VI. c. xix. p. 215. edit. Bonon. 1661.

city of Ipres and falls into the sea at Nieuport, having a *very moderate current* when the fluices upon it are open, I found its *mean declivity* to be nearly three fathoms four feet and eight inches in 20,000 fathoms of extent of its course, or very nearly one foot in a measured English mile. I say its *mean declivity*, because from what has been said above (N° 13. 27. 28. 29.) it is plain, that a greater or less quantity than ordinary of water in it will add to, or take from, something thereof; but the declivity in each part of its bed is nearly uniform.

As the sources of this river, and those of the Isere which joins it at Fort Knock, four leagues from Nieuport, are in the higher grounds of Flanders towards Houthem, Mount Kemele, Swaertsberg, Catsberg, and the other hills as far as Mount Cassel; and as the rest of their course is in a flat country with a very small descent towards the sea, the declivity thereof may be taken as a *mean* between that of the other rivers and canals of Flanders: the *artificial canals* will have less, not above a six or seven thousandth part of their extent, or one twelfth of an inch in each eight fathoms: the rivers Lys and Escaut, before they fall into the flat country, something more, after which they may have about the same, or even something less between Ghent and Antwerp.

This

This quantity of three fathoms four feet and eight inches of declivity in 20,000 fathoms of extent, gives the proportion of the declivity to the extent as 1 to 5292, which is one line or twelfth part of an inch in $6\frac{1}{8}$ fathoms, and two feet seven inches in one French league of 2283 fathoms. Now the measured English mile containing 5280 feet, this proportion of $\frac{1}{5292}$ approaches so very near to one foot of declivity in every measured mile of extent, that I shall every where reduce what I call the *mean declivity* to that quantity, as a standard wherewith to compare the rest.

74. In canals, all whose sluices and vents have been kept shut a sufficient time to render the water stagnant throughout their whole length, there cannot be allowed above an inch or two of declivity for each mile in length, on account of the water that unavoidably runs off through the chinks of the doors of sluices, drains, &c.

75. According to the observations of the Abbé CHAPPE D'AUTEROCHÉ ^(m), the floor of the Hall of the Royal Observatory at Paris is forty-five fathoms three feet and five inches French above the level of the sea at the mouth of the Seine. According to the Abbé NOLLET, this same floor is forty-six fathom above the level of the Ocean, and only forty-five fathoms above the level of the Mediter-

(m) See Relation de son Voyage en Sibirie, tom. II. p. 406, 407. 444-

anean sea. Again, according to the above Abbé CHAPPE, the said floor of the Observatory is elevated twenty-four fathoms one foot and ten inches above the level of the river Seine at Paris; therefore the level of the Seine under the Pont Royal at Paris is twenty-one fathoms one foot and seven inches above the level of the Ocean; and such also is the quantity which Mess. CASSINI have given, from their own observations and experiments, for the mean height of the Seine at Paris above the level of the sea.

Now the course of the Seine from Paris to its mouth at Havre de Grace, by following all its turns and windings, is about 90,000 fathoms in length; therefore $\frac{90,000}{21.1.7} = 4232\frac{1}{2}$ fathoms of extent for one fathom of declivity in the river Seine, or one line in $4\frac{2}{3}$ fathoms, and consequently the proportion of its declivity to its length is as one to $4232\frac{1}{2}$. It is to be observed, that the bed of the Seine is deep, and its current considerably strong.

76. By similar observations and actual levellings made upon the river Loire by M. M. PICARD and PITOT ⁽ⁿ⁾, the declivity thereof in proportion to its length is found to be as one to 3174, which is one line in $3\frac{2}{3}$ fathoms. Notwithstanding this great declivity of the bed of the Loire it is observed, that the velocity of the water therein,

(n) See Memoires de l'Acad. Royale des Sciences de Paris, pour 1730.

compared to that in the Seine, is less than it should be in proportion to their respective declivities, which is very justly attributed to the much greater depth in proportion to the breadth of the Seine, above what is found in the Loire. This last river is remarkably broad, and so shallow that in many places it is hardly navigable for boats. Now this, according to the principles laid down above (N° 27, &c.) must very much diminish the swiftness of the current, which it should otherwise have from the great declivity of its bed. In confirmation of this it is moreover observed, that in great falls of rain, which equally increase the volume of water in both these rivers, the velocity in the Loire augments in a much greater proportion than it does in the Seine; and this observation is likewise conformable to the principles above laid down (N° 12. 28. &c.).

77. The river Doux, after passing by Befançon, falls into the Saone above Chalon; the Saone joins the Rhône at Lyons. This river, from Befançon to its mouth in the Mediterranean sea, is one of the most rapid in the known world: the velocity of its current is at least double to that of the Seine or Loire, and its course is almost in a straight line. The difference of elevation of this river at Befançon, above that of its mouth in the Mediterranean sea, after a course of about eighty-six French leagues, has
been

been found, by a long series of barometrical observations, to be about seventy-five fathom ^(e), which gives the proportion of the declivity to the extent as one to 2620, or about one third of a line to each fathom. This is double the *mean declivity* of the rivers in Flanders; but the velocity of the current in the Rhone is at least triple that in the others (N° 29.).

78. From the above *data*, got from observations and actual mensuration, and from many others of the same nature too long to mention here, we may deduce the following table of comparative proportions between the declivities and velocities in different kinds of rivers.

(e) See Cours de Physique de Para, tom. II. N° 740.

Distinctive attributes of the various kinds of rivers and flowing waters.					
Rates or classes of rivers and flowing waters.	Comparative degrees of the mean velocities of currents	Seconds of time wherein currents run 20 fathom.	Fathom run by the current in 1 minute of time.	Ratios of declivity compared with horizontal length.	Fathoms of length for each $\frac{1}{16}$ inch of declivity.
1	0	0	0	$1:1000$	14 { Channels wherein the resistance from the bed, and other obstacles, equal the quantity of current acquired from the declivity; so that the waters would stagnate therein, were it not for the compression and impulsion of the upper and backwaters.
2	$\frac{1}{4}$	180	$6\frac{2}{3}$	$\frac{1}{7000}$	8 { Artificial canals in the Dutch and Austrian Netherlands.
3	1	120	10	$\frac{1}{3100}$	6 { Rivers in low and flat countries, full of turns and windings, and of a very slow current, subject to frequent and lasting inundations.
4	$1\frac{1}{2}$	80	15	$\frac{1}{1000}$	$4\frac{2}{3}$ { Rivers in most countries that are a mean between flat and hilly, which have a good current, but are subject to overflow: Also, the upper parts of rivers in flat countries.
5	$2\frac{1}{2}$	55	$21\frac{2}{3}$	$\frac{1}{3100}$	$3\frac{2}{3}$ { Rivers in hilly countries, with a strong current, and seldom subject to inundations: Also, all rivers near their sources have this declivity and velocity, and often much more.
6	3	40	30	$\frac{1}{1000}$	3 { Rivers in mountainous countries, having a rapid current and straight course, and very rarely overflowing.
7	5	24	50	$\frac{1}{1000}$	$2\frac{1}{3}$ { Rivers in their descent from among mountains down into the plains below, in which places they run torrent-wise.
8	8	15	80	$\frac{1}{1700}$	2 Absolute torrents among mountains.

I should

I should think it quite superfluous to give any explanation of a table so clear and intelligible as the above; and shall only remark upon it that *the comparative degrees of the mean velocities of the respective currents* in the second column are the result of observations and experiments, the method of making which has been given above (N^o 26.): but as the velocity of rivers is very different in different seasons of the year, which augment or diminish greatly the mass of waters in their beds, *a mean* has been kept to, as much as possible, in the above table.

By taking the degree of velocity of the current in any river, a thing so easy to be done; and observing its other characteristics as laid down above under the title of *distinctive attributes*, it will be easy to judge very nearly of *the quantity of declivity in that part of the river*.

79. After carefully comparing what has been said in the relations of travellers, and in the best treatises of geography, upon the principal rivers in the known world, I should be inclined to class them in the following manner, particularly entreating at the same time that my opinion about it may be regarded as simple conjecture, which I leave to be rectified by those better acquainted with the matter than it is possible for me to be.

Under the first rate or class in the above table I should put that part of the channel of most great rivers which
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is in extensive plains next the sea; with regard to *the declivity thereof alone*, but not at all with regard to the velocity of the current there, which is often very great from the compression and impulsion of the upper waters, as has been repeatedly shewn above must be the case (N° 29. 38. 43. 72.).

Second rate or class. Most artificial canals in flat countries, made for the use of navigation; especially those in the Dutch and Austrian Netherlands.

Third rate or class. The river Trent; the Scheld and the Lys below Ghent; the Isere and the Iprelee below Fort Knock in Flanders; many rivers in the territories of Bologna and Ferrara in Italy; the river Meander in Natolia; and innumerable others in flat countries.

Fourth class. The Thames; the Lys and the Scheld above Ghent in Flanders; the Senne, the Dyle, and the Demmer, in Brabant; the Seine and the Somme in France; the Nile and the Niger in Africa; the rivers of St. Lawrence below Lake Ontario, the Oroonoko, the river of Amazons, and the rivers of Paraguay, in America.

Fifth class. The Severn and Ouse in England; the Loire and Garonne in France; the Tagus, the Guadiana, and the Guadalquivir, in Spain; the Po and the Tiber in Italy; the Meuse, the Rhine, and the Elbe, in Germany; the Weiffel, the Neister, the Bog, and the Nieper, in

in Poland; the Don and the Dwina in Russia; the Amur or Saghalien in Tartary; the Yellow and Blue Rivers in China; the rivers of Cambodia, Ava, and Ganges, in India; the Euphrates; the river Zaire in Congo; the Mississippi.

Sixth class. The Rhone in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtysh and Oby, the Jenesea and Lena, in Siberia; the river Indus; the Tigris; the Malmistra in Cilicia.

Seventh class. In this class can only be enumerated those parts of rivers where they descend from among mountains into the plain country below; as also some rivers passing through the midst of mountains.

Eighth class. To this class belong all torrents among mountains; such, for example, as the Bourns in the Highlands of Scotland are described to be.

S E C T I O N VI.

A general and easy method of taking levels through large extents of country where rivers pass; and also of computing the heights of interior parts of continents above the surface of the sea.

80. After all I have said hitherto in this essay, and particularly in the foregoing section, what I am about to lay

lay down under this last head of it must appear very plain and easy. I am very far, however, from giving the methods I am going to propose *for taking the levels through whole countries and continents as far as rivers extend*, as strictly exact; I know very well that it is next to impossible they should be so, considering the continual variations in the declivities of rivers, and in the velocities of their currents in different parts, as also the impossibility of knowing the exact length of their course through all their turns and windings. I only give them therefore as a general and easy method of computing the relative heights of countries without deviating much from the truth, which, perhaps, is all that may be necessary for the consideration of the natural philosopher. At all events, they may be of some use, for this end, in so many parts of the earth through which rivers pass, and where no barometrical observations, or any others whatever, for taking heights above the sea, have been, or perhaps ever will be made. They may also be found useful in taking the levels through a large extent of flat countries where regular canals and rivers pass, and where the difference of elevation is too small to be observed by the barometer, and where also the taking them through so great an extent by the common methods of levelling would be much too expensive for the purposes required. Now in

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this last case I have found, by experience, that by the method I here propose the difference of heights may very easily be found, and that very near to the truth.

For this end it may be proper to premise a few necessary considerations and precautions to be observed in making use of the method I here propose. They would easily occur to any one who considers the principles whereon it is grounded; but to save trouble I shall put them down in a few words.

81. The first is, that a particular attention must be had to the quantity of water actually in the river at the time of the operation, so that according as the greater or less quantity thereof may augment or diminish the velocity of the current, allowance may be made conformable thereto in determining the quantity of declivity from the degree of velocity.

2dly, Observing this precaution throughout the whole river, or all that part of it wherein we want to find the difference of elevations, we must next endeavour to determine, as near as possible, by the principles laid down in the last section, *all the variations of declivity from the variations of velocity within those limits, and also the exact length and quantity of each.*

3dly, The same attention must be had in taking the difference of heights by canals, while their sluices and,

communications are kept constantly open, so as to effectuate a compleat natural current throughout the whole extent thereof; for in this case they are no other than rivers, and their waters follow the same laws of motion.

4thly, But in canals which are shut, and their waters kept up by sluices so as to render them nearly stagnant, the practice of this method will be different from what it is in rivers and open canals: for in this case there cannot be allowed for the declivity of the surface of the water from sluice to sluice above one inch, or two at most, in each mile of length, according as there may be fewer or more accidental drainings of the water in it (N^o 74.).

Again, as it may happen, in taking the levels of countries by the means of artificial canals, that the water in different parts may have different directions, attention must be had to *add* or *subtract* respectively the total declivity of each.

Moreover, it almost always happens, in canals where the sluices are shut, that the water on the two sides of each sluice is of a very different height, the back waters being kept up, while the lower are run off to a certain point; but in sluices next the sea, the tide against the outer gates is sometimes lower and sometimes higher than the water in the canal above. In all these cases, the difference of height must be exactly measured, and the quantity

quantity respectively added or subtracted in the account of the levelling.

5thly, After this it is necessary to determine, as nearly as possible, the length of the canals and rivers through all their turns and windings, and throughout the whole extent of country in which we want the difference of elevations. This may be done by an actual mensuration, or by the general opinion of the inhabitants of each part of the country, which, being founded upon the long and continually repeated experience of an infinity of people, will be found to differ very little from the truth, *attention being had to the quantity of their nominal measures*; even the errors in *more* or *less* will nearly compensate each other; or, finally, in great extents it may suffice to compute them from good geographical maps.

6thly, This being done by one or the other of these methods, it will be easy, from the quantity of declivity before determined for each part in particular, to find the whole quantity of declivity throughout the whole extent of country measured, or from any one part thereof to any other along the rivers or canals in question, which are supposed to be continued without interruption from one place to the other. If to this be added the relative height of the country in each place compared with the level of the water in the part of the river or canal next to each,

we shall have very nearly the difference of elevation of those two parts of the country. And thus the levels may be taken from the sea through any extent of country, nay even through whole continents, as far as rivers or canals extend without interruption. Cataracts themselves, such as those in the Nile and in the river of St. Lawrence, need not hinder the operation, since we have only to take the respective heights from which they fall into the account as we do in common fluices, and allow for the increase of velocity produced by them in the current of the river above and below the places where they exist.

82. Although I do not pretend to equal this method (of finding the difference of heights in countries) *for exactness* to the levels taken by actual mensuration, or to those found by a long series of nice barometrical observations; yet it must be allowed, that it is free from many inconveniencies, and accompanied with many conveniences, which the others are not. It may be easily carried through great extents of country, where the other methods cannot be put in practice, on account of the expence or time required; and this may be done with very little trouble, and perhaps with sufficient exactness to answer all the purposes of the natural philosopher in his considerations on the globe we inhabit. Although the method of taking heights by barometrical observations

observations is highly useful, and among mountains (where mine can be of little or no service) far preferable to every other hitherto discovered; yet it will easily be acknowledged by every one who is acquainted with what M. DE LUC ^(m), Sir GEORGE SHUCKBURGH, and Colonel ROY ⁽ⁿ⁾, have done upon this subject, that the greatest attention to an infinity of varying circumstances, as well as the greatest nicety and exactness both in the instruments and in repeated observations, are necessary if we would come at the truth thereby.

Again, the method of taking the difference of heights by the quantity of declivity in rivers requires no attention to the curvature of the globe, an object (as every one knows) infinitely too considerable to be neglected in the common method of levelling; as are also the great and varying refractions of the visual rays so near to the surface of the earth as they must be taken in the practice of that method. The quantity of effects and of errors in the visuals proceeding from this last cause must be very different at different times, as it depends wholly on the greater or less density, on the greater or less quantity of vapours suspended in the lowest part of the atmo-

(m) See his work on Barometers and Thermometers, in two vol. quarto.

(n) See the learned and curious treatises of these two gentlemen in the Philosophical Transactions for 1777.

sphere,

sphere, the state of which seldom remains long the same. Now it is no easy matter either to determine the quantity of these exactly, or to calculate the effects and errors in the visual rays proceeding therefrom, which yet must be done to come at the truth by the common method of levelling; whereas, in the method I propose, no such considerations are necessary, as is evident from the nature of it.

But this is more than enough on a method so obvious and easy; I shall now give a few examples of it, and thereby conclude this essay, already perhaps much too long.

83. Supposing the length of the Scheld, between Antwerp and Ghent, following all its meanders, to be forty measured English miles, as it is reckoned nearly to be; and supposing the length of the said Scheld between Ghent and Tournay to be fifty of the same miles; and that of the Lys from Ghent to Commines, where it approaches nearest to the city of Ipres, forty-six miles; it is required to know the respective differences of elevation between all these places.

It may be found above (N° 78. 79.) that the river Scheld, between Ghent and Antwerp, has not above one foot declivity in each mile of its course; and that the

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Scheld and the Lys, above Ghent, have about one foot declivity in each four thousand feet of length.

According to this, the surface of the Scheld in Ghent is about forty feet higher than it is at Antwerp; and at Tournay it is sixty-six feet higher than at Ghent, and one hundred and six feet higher than at Antwerp. So also the surface of the Lys at Commynes is sixty-one feet higher than at its junction with the Scheld in Ghent, and one hundred and one feet higher than the same at Antwerp. From hence it may be deduced, that the Scheld at Tournay is about five feet higher than the Lys at Commynes, through twenty-five miles of interjacent country.

84. Suppose it be required to find the difference of height between the surface of the Lys at Commynes and the surface of the canal at Ipres which falls into the sea at Nieuport on the Coast of Flanders. The distance between Ipres and Commynes is nearly seven measured miles, through which there is no communication by water; but there is one a great way round, which therefore, for the purpose required, must be followed through all the differences of elevation comprized therein, *viz.*

Descent

*Descent towards the sea at Nieuport on the Coast of
Flanders.*

	Feet.
Total declivity of the Lys from Commines to Ghent,	
46 miles, - - - - -	61
Declivity of the canal from Ghent to Bruges, when the sluices are shut, - - - - -	1
Difference of height of the water in the aforefaid canal above that in the canal from Bruges to Ostend, which two communicate together by sluices, - - - - -	8
Declivity from Bruges to Plaschendahl where the canal of Nieuport joins that of Ostend, -	6
Total declivity from Plaschendahl to Nieuport, in- cluding the difference of surfaces in two interme- diate sluices, - - - - -	8
Total declivity, all one way, from Commines to Nieuport, about 95 miles, - - - - -	84

Ascent

Ascent from the sea at Nieuport to the city of Ipres.

	Feet.
Difference of height which the river from Nieuport to Ipres has above the canal from Nieuport to Plaschendahl, taken cross the harbour of Nieuport, by means of the tides which come up against the outer gates of the sluices next the sea on each,	3
Declivity of the river Ipree from Nieuport to Boefinghe, - - - - -	11
Difference of level of the water above and below the sluice of Boefinghe, - - - - -	$22\frac{2}{3}$
Declivity in the canal from Boefinghe to Ipres, - - - - -	$\frac{1}{3}$
Total ascent, all one way, from Nieuport to Ipres,	<u>37</u>

Now $84 - 37 = 47$ feet for the difference of height which the surface of the Lys at Commynes has above the surface of the canal at Ipres. I have made use of the above example preferably to any others, as it is very complicated, and because the quantities of declivity which I have put down are not arbitrary; and, moreover, because I had the good fortune to find, some years after I had taken those measures, that I only differed two feet

from what was found by actual levels made from the Lys to Ipres by the French engineers during the time that LEWIS the XIVth was master of the country, when there were propofals for opening a canal from the one to the other.

85. I fhall venture to carry my conjectures ftill farther, and grounding them upon the principles laid down above (N° 78. 79.) I fhall take a general view of the elevations of continents along the courfe of the principal rivers in the known world. I cannot, however, repeat too often, that I give this as a matter of mere conjecture and curiofity. It has not been, nor ever will be, in my power, or in that of any other particular perfon whatfoever, to follow the courfes of all the rivers mentioned in the enfuing table from their mouths to their fources. All that can poffibly be done on this head, is to examine the relations of voyagers and geographers concerning each river as far as it is known, and to reduce it by that means within the compafs of the hydrometrical principles laid down in this effay. This is what I have done as far as I could; and therefore, allowing that I make great mistakes therein, yet I do not think that I merit much blame on that account, as I have done what I was able to do. If I am blame-worthy, it is for having launched out

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too far into the regions of conjecture; but as such mistakes as these are no ways prejudicial to my fellow creatures, to whom I wish to be useful, and as they may give occasion for others to rectify them, and so lead them to a subject which otherwise, perhaps, they might never have attended to, I shall hope for indulgence from all those who wish well to humanity and to useful knowledge.

86. A table of the elevation of countries above the surface of the sea, at each 100 miles of length up the course of the principal rivers in the world, as far as they extend; by computation from the principles laid down in this treatise.

Feet of elevation in 100 miles.	Feet of elevation in 200 miles.	Feet of elevation in 300 miles.	Feet of elevation in 400 miles.	Feet of elevation in 500 miles.	Feet of elevation in 600 miles.	Feet of elevation in 700 miles.	Feet of elevation in 800 miles.	Feet of elevation in 900 miles.	Feet of elevation in 1000 miles.	Names of Rivers, And quantities whereby the length of their course is to be diminished, to have the distance from their mouths in a direct line.
100	210	330	470	630	820	1040	1300	1600	1950	The river Trent, the Meander, and many others of the same kind, which are seldom of great extent: in these one-third of the course may be allowed for its deviations from a right line.
150	310	480	670	880	1120	1400	1770	2160	2620	
220	450	700	980	1290	1640	2040	2500	3030	3630	The river Thames; the Seine and the Somme in France; the Nile and the Niger in Africa; the river St. Laurence, the Oronoko, the river of Amazons, the rivers of Paraguay: in these about one-fourth of the length of course may be allowed for turns and windings in it.
										The Severn; the Loire and Garonne in France; the Tagus, Guadiana and Guadalquivir in Spain; the Po and the Tyber in Italy; the Meuse, Rhine and Elbe in Germany; the Weixel, Neister, Bog and Nieper in Poland; the Don and Dwina in Russia; the Amur in Tartary; the Hoang-ho-keou and Yang-tse Kiang-keou in China; the rivers of Cambodia, Ava and Ganges in India; the Euphrates; the Zaïre in Congo; the Mississippi: in these may be allowed about one-fifth of the length of the course for turns and windings in it.
300	650	1050	1520	2070	2720	3500	4440	5570	6920	The Rhône in France; the Ebro and Duero in Spain; the Danube; the Volga; the Irisk, Ob, Jeneca and Lena in Siberia; the Mal, mitra in Cilicia; the Tigris; the Indus. The course of these rapid rivers is usually very straight, and there cannot be above one-fourth of the length thereof allowed for deviations from a right line.

XXXVIII. *Extract of two Meteorological Journals of the Weather, observed at Nain, in 57° North Latitude, and at Okak, in 57° 30', North Latitude, both on the Coast of Labradore. Communicated by Mr. De la Trobe to the President, and by him to the Society.*

Read May 20, 1779.

THE thermometer, of FAHRENHEIT's scale, was observed at eight in the morning, at noon, at four in the afternoon, and at eight in the evening.

The barometer, whose scale is French measure, was observed at eight in the morning, and at eight in the evening.

Months.

Months.	Thermometer at Nain.			Barometer at Nain.			Thermometer at Okak.		
	Highest	Lowest	Mean.	Highest	Lowest	Mean.	Highest	Lowest	Mean.
1777	°	°	°				°	°	°
August	76,0	36,0	52,3	28,3	27,6½	27,10,4	74,0	36,0	52,0
Septemb.	72,0	27,0	43,6	28,5½	27,6½	28,0,4	68,0	21,0	43,9
October	51,0	12,0	30,2	28,7½	26,11½	28,0,1	48,0	13,0	30,0
Novemb.	41,0	8,0	15,4	28,9	26,9	28,0,0	39,0	5,0	17,5
December	31,0	26,0	10,3	28,5½	26,3	28,3,0	32,0	28,0	4,2
1778									
January	18,0	30,0	12,2	28,5	27,1½	28,4,5	16,0	27,0	11,0
February	32,0	27,0	10,7	28,6	27,3	28,7,6	34,0	25,0	9,2
March	27,0	25,0	4,3	28,10½	27,2½	27,4,0	32,0	18,0	6,5
April	49,0	8,0	27,1	28,6½	27,6	28,4,3	53,0	10,0	28,5
May	68,0	29,0	39,0	28,5½	27,7	28,7,5	65,0	27,0	37,5
June	77,0	33,0	45,0	28,6	27,4	28,0,0	73,0	31,0	45,7
July	82,0	34,0	51,0	28,3½	27,8½	28,0,1	81,0	36,0	51,3
August	74,0	40,0	52,5	28,5	27,5½	28,0,0	72,0	41,0	52,0
Septemb.	69,0	34,0	43,5	28,0	27,10	28,2,0	53,0	32,0	43,0



I M P R O V E M E N T S

I N

E L E C T R I C I T Y.

By JOHN INGENHOUSZ, F.R.S.

BODY PHYSICIAN TO THEIR IMPERIAL MAJESTIES.

Who was nominated by the President and Council to
prosecute Discoveries in Natural History, pursuant to
the Will of the late HENRY BAKER, Esq. F. R. S.

Read at the ROYAL SOCIETY, June 3, 1779.

IMPROVEMENTS IN ELECTRICITY.

BEING appointed to deliver the annual dissertation on some philosophical subject, instituted by our worthy brother the late Mr. HENRY BAKER, I will endeavour to explain some contrivances in electricity which I invented a good while ago, one of which has been much employed, and has undergone several material improvements since I first thought of it.

IT is now about fifteen years ago since I began to make use of flat glasses instead of globes or cylinders, to excite electricity. Finding that a greater quantity of electricity could be excited upon a flat piece of glass, when rubbed on both surfaces, than when it was only exposed to friction on one side; I thought it would be an advantage to substitute a round plate, or disk, of glass, to a globe

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or cylinder. I also thought another material advantage might be derived from a plate of glass, as the form of it admits of placing cushions or rubbers upon different parts of it, and taking the electricity, excited by these rubbers, from the interstices between them, which cannot conveniently be done when a globe or cylinder is used. The only inconvenience which I at first conceived would ensue from it was, that the center upon which the plate was to be fixed and whirled round, would always be too near the rubbers (unless these were very short, or the plate of a considerable size) and that these would throw the electricity, collected on the surface of the glass, upon that very center. In order to obviate this difficulty, I proposed to make the center likewise of glass, or some other non-conducting substance, as, for instance, baked wood.

I began first by making use of one of those glass stands, which they call a waiter, and which has a glass support fixed at right angles to its center. I whirled the waiter round as well as I could, rubbing it sometimes on one side, sometimes on both. In this imperfect state I shewed it to Dr. FRANKLIN, who approved much of the scheme, and advised me to pursue it. Soon after, I shewed it to several of my acquaintance, and in a short time I found such machines ready made at Mr. RAMSDEN'S and some
2 other

other mathematical instrument-makers Since that time great use has been made of these electrical plate machines throughout Europe, as they were thought by many to be more powerful in a little compass than those with globes and cylinders.

In my travels through different countries, I now and then met with considerable improvements made in them. Abbé FONTANA had contrived one for the cabinet of the great Duke of Tuscany, which consisted of two plates, eighteen inches diameter, fixed to the same center, and each rubbed on both sides on two opposite places. The electrical fire excited on these joint plates, when forced upon a conductor divided in two branches to receive it, was very powerful; so much so, that the conductor, being unable to contain the whole, threw it back upon the brass center; from which it passed to the hand of the operator, and gave him a very disagreeable shock.

Mr. CUTHBERTSON, an ingenious mathematical instrument-maker at Amsterdam, contrived an apparatus with a double plate, by which all the electrical fire collected by the eight cushions was forced upon the conductor, so that none of it could be thrown back upon the center, though made of brass. His contrivance consisted in placing between the glass plates a strong glass ring, about two inches in diameter, so that the brass

center passed through the middle of it. This ring was stuck to the plates with sealing wax or some other non-conducting cement; and the space between the center and the ring was carefully filled with the same non-conducting substance. The conductor had two branches, each of which was placed between the two glasses, reaching very near to the glass ring. By this method all, or almost all, the electricity excited by the eight cushions, was forced to pass upon the conductor, there being no way to reach the brass center, between which and the conductor all communication was cut off by the above mentioned glass ring being filled up with a non-conducting cement. The power of such a machine, notwithstanding that the plates were not above fifteen inches diameter, was very astonishing. I saw one made in London with this improvement (the glass plates being eighteen inches diameter) by which a coated jar of two quarts was fully charged in less than five seconds.

Mr. C. CUYPERS, an ingenious electrician at Delft, has not a little contributed to the improvement of these machines, by making them less liable to be affected by damp weather. This gentleman, considering that all glasses are not equally fit for electricity, and that J. H. WARTZ, and after him Professor MUSSCHENBROEK, were of opinion, that glass, in the composition of which there enters

a great deal of alkaline salt, is very apt to attract moisture from the air, and therefore less proper for electricity (which defect they thought might be corrected by exposing it to a violent and continued heat), took a proper advantage of this knowledge in the improvement of the machines with flat glasses. He found, that glasses, which have been many years exposed to the warm air of a room, very old looking glasses for instance, become by this means much harder, so as better to resist the force of a file; and are then much better for electrical machines: and that such glasses become still incomparably better, if they are exposed to a considerable degree of heat during some months: the heat forcing out of the glass (at least out of its surface) the alkaline salt, not vitrified, which is to be found upon it, and may be known by the taste.

In December 1777 I saw one of these double-plated machines at Mr. CUYPER's house, and found it do admirably well, though the weather was at the time very damp, and the machine kept in a room in which there never was any fire made, and though the cushions had no amalgama upon them: they were made of yellow Turkish leather, stuffed with fine shavings of cork, rammed in them; and had been pressed to give them an equal smooth surface.

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The same gentleman found glasses, prepared in the above mentioned way, far superior in strength to a cake of rosin used in the electrophorus. He published his method in a pamphlet, intitled, *Exposé d'une Methode, par laquelle on rend les Disques de Verre destinés à des Machines Electriques capables d'exciter l'Electricité dans une Air humide, suivi d'une Maniere de faire de très bons coussins pour frotter les Verres des Machines electriques, et de la Description d'un Electrophore perpetuel plus parfait que ceux dont on s'est servi jusqu' ici.* A la Haye, 1778.

Those who make use of plate machines should carefully avoid putting the apparatus near the fire, for the purpose of drying or warming it; because the sudden expansion of the glass by the heat cannot so quickly propagate itself through its whole extent; for the center being commonly squeezed between two flat shives of brass, with a piece of leather between the metal and the glass, does not acquire a similar degree of heat at the same time as the rest, and cannot so easily expand; and therefore the plate is in great danger of breaking. If, in consequence of such a blunder, a flaw should happen, its progress might be stopped by drilling a round hole at the extremity of the flaw. These flat glasses may very safely be rubbed with a dry warm cloth.

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As the quantity of electricity excited upon glass is nearly in the proportion of the surface exposed to friction; and as glasses of a great size are very precious, and liable to accidents, I conceived, that instead of a disk of flat glass one might substitute one of paste-board, thoroughly imbibed with copal or amber varnish.

To try how this would answer, about seven years ago I ordered three paste-board disks to be made, of four feet in diameter, the distance of six inches from the center being the fittest to give the whole a proper support in whirling it round. When these disks were thoroughly dried and heated, I poured upon them a varnish made of amber dissolved in linseed oil. After they had taken in as much of the varnish as they could imbibe, I covered them with a thick coat of the same varnish, and dried them by the heat of a German stove.

When the varnish was very hard, I found, that even a slight friction with a cat's skin or hare's skin excited a strong electricity upon them.

I then made a frame to place them in, and to whirl them round; which frame was so contrived, that it could contain about twelve such disks, whirling all round on the same center. It consisted of two square pillars of wood, about five feet high, and three inches broad; joined together at top and bottom by a transverse piece
of

of wood. In the middle of the two pillars was a hole, about an inch and an half diameter, fitted to receive a wooden axis, which could be placed in, and taken out, at pleasure. Upon this axis were to be stuck the paste-board disks; and a flat board, three inches broad, covered on both sides with flannel, and over this with a hare's skin, was to be placed between each paste-board. The two square pillars were also to be wrapped up first with flannel, and over that with hares skin.

The flat boards, to be placed between the paste-board disks, had each a notch in the center, to give room for the axis to turn round freely. These flat boards could be brought as near one another as was required by two wooden male screws, placed at the upper and lower end of the frame, which reached from one square pillar to the other; which screws were to receive a notch cut out at the upper and under end of each flat board, in order to keep them steady in their vertical situation. A female screw, turning upon these horizontal male screws, was placed between each of the flat boards at their upper and under extremities, and served to bring each of the disks as near in contact with the hares skins as was required to receive a proper friction.

The three paste-boards were fixed in the frame, and whirled round. The electricity excited was so strong
that

that I took sparks between one and two feet long from the front surface of the first disk by approaching my knuckle to it. I then applied a tin conductor to it, about six feet long and six inches diameter, divided into two branches, the extremities of which were furnished with a thick silver lace fringe instead of points. The sparks from this conductor were about four or five inches long, appeared to be very thick, were very brilliant, and so strong, that I did not chuse to receive many of them; nor did those who came to see the machine care to receive more than one. As these sparks succeeded one another at short intervals, I think they would have been much longer if every thing had been adapted for that purpose; as I saw afterwards done at Mr. NAIRNE's, who contrived to obtain sparks of twenty inches long and upwards from a large glass cylinder.

I considered this paste-board machine rather as a rough sketch of an apparatus, by which I conceived the hope of obtaining an electrical power of almost any degree required, than as a compleat machine. My intention was to find out a contrivance by which a very great quantity of electrical fire might be collected without great expence and without much danger of breaking the apparatus, which two articles cannot be avoided when you make use of uncommon size glass cylinders or disks.

I saw, two years ago, at the Duke of CHAULNES at Paris, an apparatus which had a plate glass five feet diameter. This alone cost him eight hundred French livres.

As I had not adapted the tin conductor to receive the electricity from the three disks, but only from the front disk, I cannot tell whether the force of electricity would have been proportionably stronger if I had made some metallic communication between each of the disks.

I found, that the apparatus, as it was constructed, could not easily have admitted more than three such large disks; for the twelve surfaces exposed to friction being each a foot and an half long, and above three inches broad, made so much resistance to the working of the machine, that it required a strong arm to work it.

Being satisfied with having found that by this, or a contrivance of the same kind, a much greater power could be excited than by the common glass apparatuses; I did not chuse to put myself to more trouble or expence to increase the strength of its frame, or the number of disks.

I must observe, that such a machine may be kept in good order in a heated room; but that it will soon lose its force in countries where it is not the custom to heat rooms as the Germans do. My paste-board disks kept

very good during all the time I left them in a room constantly warmed, which was about two months; but, when they were placed in a cold room, they soon lost their power, having probably attracted moisture from the air. I cannot be sure that the varnish I got made for the purpose was of the best kind: I have reason to suspect the contrary, and therefore I should think that much better might be obtained in London.

Such paste-boards might be possibly preserved from attracting moisture, by keeping them shut up in a box made on purpose, lined within with tinfoil. The moisture might also be expelled again by placing them a good while in a heated room, or upon a baker's oven.

It seems, besides, not improbable, that a kind of paste-board might be made by sticking together the lamina of paper (first thoroughly dried) with a good oil varnish instead of common paste, as this last never can be deprived of its watery particles without losing its cohesive quality. A good kind of paste-board might likewise be contrived by sticking together silk cloth instead of paper.

I contrived a plated machine, the disk of which was made of baked wood and boiled in linseed oil; but it did not answer near so well as the paste-board disks.

I found the paste-board disks not much less susceptible of electricity before I had varnished them than they were afterwards; but they lost their force again in a few minutes, and did not recover it till they were dried and heated again.

It is well known, that writing and brown packing paper, when warmed, may acquire a considerable electrical power by being rubbed with hares skin, a piece of wood, ivory, nay even (as I found by experience) with a metallic body.

As it seems to be a general law of nature, that all resinous bodies, excited either with a positive or a negative electricity, are more tenacious of keeping it, or seem to part with it, as it were, with more reluctance than glass (as I have demonstrated in a paper, read last year before the Royal Society); it is adviseable, that the conductor of such a paper machine be not furnished with metallic points, which being necessarily kept at a distance will not take away all the electricity; but that some flexible, conducting substance, as silver or brass lace fringes, communicating with the conductor, be in close contact with the excited surface.

As woollen cloth or Manchester cotton velvet, and such like substances, excite a good deal of electricity upon dried paper and resinous substances, and do not wear out
so

so soon as hares skin, it might, perhaps, be found better to substitute them for hares skin.

I have also excited a very considerable electrical force on strong silk velvet, tied upon the circumference of two wooden disks, two feet in diameter, and distant about three feet from one another, fixed upon a wooden axis. The velvet was supported by a strong silk cloth tied under it, in order to give it more strength and steadiness. This machine had the appearance of a drum, and was whirled round, as is usually done with glass cylinders. The rubber was a cushion covered with hares skin.

As silk cloth, and more particularly oiled silk, very easily receives a strong electricity, I make no doubt but a good use might be made of them, by exposing a great surface of them (which may be as large as one pleases) to friction. I have attempted more than one method of constructing such a machine; but as I tried it only in small, I have not pursued the object far enough, and therefore, I think, I have no right to throw out hints unsupported by experience.



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E R R A T A.

V O L. LXVIII. P A R T II.

Page Line

1040. 12. *for conducting, read non-conducting.*

V O L. LXIX. P A R T I.

18. 19. *for wetted with antimony, read heated with antimony.*
16. 15. *for an ounce and a half, read a drachm and a half.*
24. 5. *for 5 ounces 36 grains, read 5 drachms 36 grains.*
27. 4. *for Zeolite, read Zeolite.*
30. 19. *for tin, read fire.*
122. 10. *for percursio, read percussio.*
— 14. *Idem.*
— 19. *for percursionibus, read percussionibus.*
124. 15. *for percursiones, read percussiones.*
126. 12. *for percursione, read percussione.*
— 20. *for percursionis, read percussiois.*
131. 14. *for it may observed, read it may be observed.*
182. 2. *for Stephens, read Stevens.*
243. last line of the English, *for pea, read pen.*

V O L. LXIX. P A R T II.

364. column the 4th, *for Mr. de Luc's thermometer, read Mr. de Luc's rule.*
373. column the 6th, line the last, *for 96, read 98.*
Ibid. column the 7th, line the last, *for 0, read +0, 2.*
380. 15. *for open on both sides, read open at both ends.*
540. *for working-needle, read sewing-needle, passim.*

I N D E X.

684. *for Eric Prosperin, read Prosperin Eric.*
685. 22. *for pistol to make it, read pistol with it.*
690. 16. *for one for making, read one for firing.*
692. 30. *for wetted, read melted.*

